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A VAST and unsettling transformation in the writing of history is taking place in America. During the past several decades we seem to be experiencing nothing less than what Daniel Rodgers, in chapter 1, calls “a historiographical revolution.”¹ The historical landscape seems to be shifting, and only now are we beginning to assess the implications of this shift. This volume, which explores many of the various ways American historians have imagined the pasts of their own country as well as other nations, is part of the assessment.

American historians have traditionally brought to bear on the past a perspective often strikingly different from that of historians of other nations. None of the major European countries, for example—neither France, Italy, Germany, nor England—has shared America’s idiosyncratic conception of Western civilization. But America’s unusual perspective on the past, both of Europe and of itself, is changing and changing radically, and these changes make this volume possible. Only at this moment—when the identity of the United States and the discipline of history are shifting in profound ways—are we able to perceive clearly the peculiar ways Americans have written about the past. Only now, it seems, could the papers included in this book have been written.

Although Americans are supposed to be a notoriously unhistorically minded people, they have always spent a great deal of time and energy writing histories of themselves and of other peoples. The United States today has a huge and diverse historical profession, and much of that profession has developed in the past thirty or forty years. In 1950 the American Historical Association had about 5,500 members. By 1960 the number had jumped by half, to more than 8,200. Ten years later it more than doubled, to about 18,000. Although by the mid-1990s membership had dramatically declined, suggesting the economic uncertainties that presently plague the American academy, the American Historical Association today still has nearly 13,000 members, with many more thousands of professionally trained historians choosing for one reason or another not to join. In addition, of course, there are many amateur historians, genealogists, and antiquarians. It is hard to imagine that many other nations produce as many history books; probably none devotes as many resources to generate historical knowledge. What is more, Americans do not study merely their own country’s past. Two out of three professional historians in the United States actually work on the histories of other societies and cultures. In fact, 590 out of the 800 Ph.D.s in history awarded by American universities in 1994—nearly three out of four—were in fields other than American history.²

This breadth of interest in other peoples' pasts is not new; it goes back at least to the beginning of professional history writing at the end of the nineteenth century. Indeed, from the very beginning of the nation Americans have been fascinated with the history of Europe and their peculiar relationship to it. That relationship, more than any other single thing, has decisively affected the character of nearly all American history writing, about both the nation itself and other nations. From the very outset the perception of a contrast with the Old World created that peculiar sense of American difference and distinctiveness which some recent historians, including some in this volume, have labeled American "exceptionalism."³

Until quite recently many Americans thought of their history and their role in the world as not merely different from those of other nations but as "exceptional"—as a beacon or model for other nations, with a special and unique destiny to lead the rest of the world to freedom and democracy. Many historians shared this view. For more than two centuries, much of the interest Americans have had in the pasts of other peoples and other cultures—in Antiquity, the Middle Ages, the Renaissance, the Reformation, in what came to be called "Western Civilization"—grew out of their desire to bolster and make sense of their "exceptionalist" destiny in the world. Surely every nation has its own peculiar view of its role in the world, but few have equaled America in promoting the claim of its special destiny. In the second half of the twentieth century only the Soviet Union could advance claims as sweeping as those of the United States about its peculiar historical mission.

In one way or another, this theme—the long-held notions nurtured by Americans and American historians about their nation's allegedly exceptional history—links the papers that follow and gives this volume a measure of its unity. As Dorothy Ross notes in chapter 4, exceptionalism for Americans has generally meant a New World that was "antithetical to the Old," an America that was different from Europe, inferior in many ways, but at least free from Europe's ills and an exemplar or model for the future progress of liberty and democracy. Exceptionalism has been a resilient long-term constant in American culture, as a number of American historians, including John Higham, Michael Kammen, and Jack P. Greene, have recently noted.⁴ Greene, in his book *The Intellectual Construction of America*, traced the term exceptionalism back to Tocqueville's statement about Americans being "quite exceptional." After surveying American thinking over the past three centuries, Greene concluded that the concept of exceptionalism was "present at the very creation of America."⁵ By the time of the Revolution this belief in its exceptionalism and its special place in the world had become an integral part of America's identity. Always underlying this belief, of course, was a sense of difference from Europe. Generation upon generation of intellectuals and members of the general public shared this conviction.

During the colonial era that sense of difference was usually one of inferiority. Most colonists realized only too keenly that they were simply British provincials living on the very edges of Christendom. They were awed and mortified by the contrast between their own seemingly trivial world and that of the great British

metropolis three thousand miles away. The Revolution changed much of this sense of inferiority. At a stroke, what had been seen as deficiencies for the colonists—their lack of a royal court, a hereditary aristocracy, and an established church—were transformed into advantages for the new republican government. Americans now saw their country possessing a freer, more prosperous, more egalitarian society than any in Europe; America had become for them a beacon and an asylum for the oppressed of the Old World. Americans in 1776 may have felt culturally inferior to Britain and to Europe; but they were a rising people, and they believed that sooner or later they would become the greatest nation in the world.

This belief by Americans that, in the words of President James K. Polk, their history lay ahead of them colored much of their national history-writing in the nineteenth century and gave it much of its teleological and exceptionalist character—its sense that the United States was the fulfillment of all that was great and progressive in the past. In chapter 7 Gordon Wood shows that if a history of the colonial period did not point to the future greatness of the United States, then few people were interested in it. It has been the same with the Civil War, which, as George Fredrickson says, “has inspired more scholarship than any other nineteenth-century subject” precisely because it defined the nation as no other nineteenth-century event did. Indeed, the subject of the Civil War is especially attractive in the present, writes Fredrickson, in that “it provides a persuasive argument for the uniqueness of American history that is not based on some claim to special virtue.”

Despite this celebration of their national history, Americans never lost sight of their history’s rootedness in the European past. Sometimes they stressed the contrast of America with Europe. Their fascination, for instance, with what Richard Kagan calls “Prescott’s paradigm”—“an understanding of Spain as America’s antithesis”—grew out of their need for an example of what they must avoid if they were to remain free and grow and prosper. The early nineteenth-century historian William Prescott, writes Kagan, “sought to determine the forces that destined certain societies for greatness, others to decadence and decay.” Americans were convinced that their republican government, liberal Protestantism, and bold commercial spirit gave their country a huge moral advantage over Spain, saddled as it was with royal absolutism, Inquisition-dominated Catholicism, and economic backwardness. In responding to this gloomy assessment of Spain, however, Americans never forgot that they were as liable to corruption and decay as the Spanish or as any people; their experiment in free government was always problematic, always capable of failing.

The New World may have been different from the Old World, but few Americans ever doubted that they were linked to that older civilization and had something to learn from it. Although they were confident that they were in the vanguard of history and were leading a corrupt monarchical world toward free republican government, they always retained a pervasive sense of their cultural inferiority to Europe. They tried to tell themselves that the arts and sciences were sooner or later destined to cross the Atlantic and flourish in the New World. In

the eighteenth century they had thrilled to Bishop George Berkeley's conventional notion of the westward movement of the arts from the Near East to Greece, from Greece to Rome, from Rome to western Europe, and from western Europe eventually across the Atlantic to their New World. They had believed, as Philip Freneau and Hugh Henry Brackenridge declared in their 1771 Princeton commencement poem, "The Rising Glory of America," that they would in time have their own Homers and Miltons and their own achievements "not less in fame than Greece and Rome of old."⁶ They had hoped that the torch of Western civilization would be passed to them, where it would shine with new brilliance.⁷ But many of them soon came to realize that the torch was not crossing the Atlantic and that amid the crass money-making of American society they would have to struggle to keep alive any semblance of the lights of European art and culture.

Whatever else the American Revolution was, it was not a repudiation of European culture. The revolutionary leaders never intended to reject the Western republic of letters; rather, they aimed to embrace it and fulfill its highest cultural aims. In later generations, however much Americans might set themselves apart from Old World politics, they never imagined that their culture was separate from that of the Old World. In his textbook, written in the early years of this century, Carlton Hayes expressed this very idea when he wrote that Europe was "the seat of that continuous high civilization which we call 'western'—which has come to be the distinctive civilization of the American continents as well as of Europe."⁸ Indeed, Eugen Weber in his chapter on the cult of "Western Civ" in American higher education writes that Americans in a peculiar manner made the Old World part of their national past. Like Europeans, writes Weber, Americans teach and write a great deal about their own national history. But unlike Europeans, Americans spend much time and energy on other peoples' histories as well. They "conceive and present the national past as prolonged backward not into the British past alone, but into a broader Western tradition, originating in the Fertile Crescent, and the Mediterranean of Greece and Rome, where the groundwork was laid for references and memories that resurface in the conflicts and creations of the Middle Ages, Renaissance, Reformation, Enlightenment, and so on." American historians invented the conception of "Western Civilization" and made it "the inescapable background" of their national history. Always American scholars have had a sense of their nation as the culmination of a long and great Western tradition, and this sense has underlain their extraordinary efforts to attach the history of the Old World to their national history. Witness an example cited by Philip Benedict in chapter 14—the course taught by Herbert Darling Foster at Dartmouth for many years: "The Puritan State in Geneva, England, and Massachusetts Bay."

Despite their differences of approach and interpretation, the authors in this volume who write on the history of Europe show that most prominent American historians over the past century have always believed that the study of the European past illuminated some aspect of American history. It did not matter whether they studied Antiquity, the Middle Ages, the Renaissance, the Reformation, or the Ancien Régime. Europe was the ground where ideas vital to America originated

and were first tested. In some cases, Americans looked to the European past for inspiration and for lessons crucial to their moral, political, and aesthetic well-being. In others, the study of Europe's history could impart cautionary lessons, tell of ideals and revolutions gone astray, of morally robust societies undermined by wrong political turns, or of economies corrupted by the excessive selfishness of individuals or classes. In all cases these American images of Europe dwelled upon America's deep European roots. Believing that they belonged to the same moral universe, were bound by the same historical rules, and were subject to the same rewards and penalties as other peoples, these American historians sought to derive both positive and negative lessons from the study of Europe. They imagined themselves not only to be a part of Western civilization but to be its fulfillment. Europe had created Western civilization. America had the responsibility of helping it survive and flourish.

Many of the essays in this book point to the intensity with which the history of Europe was studied in America, and to the link—often drawn explicitly, but nearly always implicit in historians' questions and approaches—between the European past and American society. Early in the twentieth century, as Richard Saller shows in chapter 11, Tenney Frank, the first eminent professional American historian of ancient Rome, thought that Americans and the early Romans of the republic "were kindred spirits." Since Rome's and America's moral destinies were inextricably intertwined, Frank was understandably curious about the reasons for the Roman republic's decline. He attributed that decline to the migration into freedom-loving Italy of "impulsive and passionate races that had never known self-government." It was an explanation that came right out of the anti-immigrant atmosphere of early twentieth-century America. It was as if ancient Rome and twentieth-century America were not separated by two millennia.

In her chapter devoted to the Middle Ages, Gabrielle Spiegel stresses a similar sense of connectedness between America and the distant Western past. Precisely because the United States had no medieval past, American medievalists, she writes, insisted "in a highly overdetermined fashion" on the importance of the Middle Ages to "the origin of the modern, hence American, world." From Charles Homer Haskins to Joseph Strayer, a distinguished line of American historians of the Middle Ages resolutely stressed "the continuity of the American present with past medieval institutions." In an address to the American Historical Association in 1923, Haskins argued that European history is "of profound importance to Americans. . . . We cannot ignore the vital connections between Europe and America, their histories [are] ultimately but one."⁹

This theme of America's close involvement with Europe is captured in several other chapters in this volume. Anthony Molho shows that for well over a century the Renaissance has held "a place of special honor" within the long tradition of Western civilization. Not only did Americans believe that their country could learn from fifteenth-century Florence how to convert its mercantile energies into artistic beauty, but, as Molho says, they came to see the Renaissance "as the key, pivotal moment in the unfolding of that historical process which culminated in the creation of the United States." Those Americans searching for the sources of

individualism and other modern forces that defined the special character of American society inevitably looked back to the Renaissance. It is thus not surprising that within a little more than a year of its English translation in 1878 Jacob Burckhardt's *Civilization of the Renaissance in Italy*, which set forth the clearest and most authoritative claim regarding the modernity of the Renaissance, was being used in the classrooms of Brown University.¹⁰

The Reformation was thought to be even closer still to America's spirit. Indeed, in perhaps no other field of European historiography was the sense of an intimate moral community between America and Europe more explicit and persistent than in Reformation history. Philip Benedict writes that "to this day, a powerful impetus attracting scholars to this subject remains the concern of Christian believers [in America] to explore the roots of their diverse traditions." Just as social, political, and aesthetic considerations dominated in American historiography of Antiquity, the Middle Ages, and the Renaissance, so did powerful confessional interests affect American historiography of the Reformation. As Benedict writes, more than a few American historians were willing to follow Preserved Smith in imagining the Reformation as "the origin of the modern world," whose most magnificent and accomplished product, of course, was America.

Until recently American historians have treated the French Revolution differently from the way they have treated the Renaissance and the Reformation. They have generally pictured it negatively, as something uncongenial to the American spirit. Many American historians, as Keith Baker and Joseph Zizek point out in chapter 16, have tended to draw implicit or explicit contrasts between the excesses of the French Revolution and the moderation of the American Revolution. Still, even in the case of the French Revolution some Americans wanted to place the event into some larger progressive scheme in which America was the leader. Thus as early as 1906 James Harvey Robinson urged Americans to view the French Revolution as an aspect of the great reformation that brought the world into modernity. Early twentieth-century champions of the "new history" like Robinson wanted their historical knowledge to be socially useful: this view of the Revolution as a reformation, said Robinson, would help in "our great contemporaneous task of human betterment." And that task of human betterment would best be carried out in America.

Many of this book's chapters describe the intense interest of American historians both in the distinctiveness of America and in the pasts of European countries and peoples. Since the nineteenth century, American historiography has played many variations on these two themes—America's distinctiveness from and connectedness to Europe. But the twentieth century has seen American involvement in two world wars that have complicated these themes and challenged America's older provincial sense of its relationship to Europe.

As Volker Berghahn and Charles Maier point out in chapter 17, America's participation in World War I and the subsequent debate over "war guilt" divided the American historical profession and raised questions about America's traditional connection with Europe. In the 1920s, as Philip Gleason indicates in chap-

ter 6, American professional historians first began devoting serious attention to immigration, which further unsettled conventional attitudes toward America's ties to Europe. Most important, however, in broadening America's mental horizons was the great "intellectual migration" of the 1930s that led thousands of European scholars to seek refuge in the United States from the horrors of racial persecution and war. Although the story of this migration has often been told, its effects appear to be no less astonishing in its retelling by several of the writers in this book.

All aspects of American life felt the impact of these European exiles. They enriched everything they touched, including historical scholarship. They not only profoundly transformed fields such as Medieval and Renaissance history and the history of modern Germany, but they affected history-writing in many other areas as well, including American history: think, for example, of Felix Gilbert's important book on Washington's Farewell Address.¹¹ Because they were not emigrants but exiles, forced to flee from the evils of Nazism, most of these scholars could at first only admire the country that had given them refuge and emphasize its exceptionalist difference from a brutal and corrupted Europe. But in the end all their intellectual training, all the sources of their outlook on the world, were based in Europe; and thus eventually they could not help but erode American parochialism and enlarge the perspectives of American historical scholarship.

World War II itself had a broadening effect. Of the many soldiers returning home to complete their education, many chose to study the history of countries and cultures they had encountered during the war. If the First World War had brought Germany within America's orbit of historical interest—as Berghahn and Maier's chapter demonstrates—then the Second World War brought in the entire world. America's triumphant position at the end of World War II and its new global responsibilities emphasized the two themes of distinctiveness and connectedness in American history-writing in new ways. The well-being of the country, its ability to discharge its new international responsibilities, its willingness to dedicate huge quantities of material resources to strengthening friendly "democratic" governments in all corners of the world—all required scholarly support and explanation.

America's strategic interests demanded that historians now study parts of the world they had rarely studied before. They did so with remarkable energy, enthusiastically shouldering the responsibilities they were convinced the world had thrust upon them. Centers for the study of the Soviet Union, Latin America, East Asia, Africa, and, more recently, the Middle East were set up, with each center inevitably engaging the services of one or more historians. Although small beginnings had been made before or during the war, only in the immediate postwar decades did Americans devote large intellectual energies and material resources to studying the pasts of these distant and alien cultures, all generally in the name of current policy interests.

While Martin Malia's chapter deals only with the development of Russian and Soviet history in the post-World War II United States, what he says about his

subject seems to hold for American history-writing about other non-European regions as well. Certainly Carol Gluck's account of the rapid expansion of Japanese studies in the 1950s and '60s suggests as much. Despite the strenuous efforts in the postwar era to investigate non-European history, both Malia's and Gluck's chapters also show that American historians were not always successful in fully understanding the new foreign pasts they had set out to study. But at least they had begun to break out of their earlier exclusive embrace of the western European past. Prior to the Second World War American historians had imagined their country connected in direct and indirect ways only to the history of Europe. In the past half century, they have greatly increased the links between the United States and the world beyond Europe. Indeed, American historians virtually invented the concept of "world history."

America's direct twentieth-century involvement in the world had other broadening effects on the historical profession. The postwar period was awash with social science theories that had their origins in the Old World. As the several reports of the committees of historiography of the Social Science Research Council issued in 1945 and in 1954 indicated, many distinguished members of the historical profession were eager to use "ideas and methods dominant in the various social sciences" in order to further cooperation between history and the other social sciences and thus promote "greater understanding of how men and societies change and develop through time."¹²

Although some historians, notably Charles Beard, had long been interested in European social science theories, it was only in the immediate postwar years that the American historical profession became widely and deeply engaged in these theories, especially in the ideas of Marx, Max Weber, and the *Annales* school.¹³ Many historians responded to the reports of the committees set up by the Social Science Research Council and became preoccupied with the ways social science theories might be used in history. Both Dorothy Ross and Naomi Lamoreaux discuss the rich theoretical experiments of the American historical profession in those years. The story of the Social Science History Association described by Ross exactly parallels the story of the Econometric Society described by Lamoreaux. In both cases young historians launched highly focused professional associations to promote what they believed were better, more scientific and technical approaches to the study of history. With the aid of sociologists, economists, political scientists, and anthropologists, they hoped to experiment with social theories and build economic models and thus make history quantitative, objective, and scientific. It did not take long, however, for these scientifically minded historians to realize that their organizations had become what Lamoreaux calls "intellectual ghettos" cut off from the mainstream of American history-writing.

Despite the extraordinary influence of a variety of European and cosmopolitan theories and perspectives, however, the mainstream of American historiography in the 1950s and early 1960s remained obsessed with Cold War ideas of American exceptionalism. As James Patterson notes in chapter 9, American historians of that time "tended to celebrate the stability of America's institutions, to extol its courageous role in the world wars and the Cold War, and to imagine that social

'consensus' was blurring age-old divisions of race, class, ethnicity, and religion in the nation." Ultimately their wider involvement in the world was not enough to break Americans free from their deeply rooted belief in their own uniqueness. It took a series of fundamental changes in the nation's society itself to destroy the "consensus" that Patterson speaks about and to shake historians out of their traditional conceptions of America and its relationship to the rest of the world.

These changes first became manifest in the 1960s. The civil rights movement allowed the voices of black Americans to be heard in increasing numbers. This together with the remarkable growth of immigrants from non-European areas of the world—from Mexico, other parts of Latin America, and Asia—created a new sense of American diversity and pluralism that have brought into question older conceptions of America's identity. These changes in turn have contributed to a dramatic democratization of higher education. Never before have so many Americans from so many different social and ethnic backgrounds gone to college, earned higher degrees, and studied history.

The women's movement has had equally powerful effects on the history profession. The number of new female Ph.D.s in history steadily grew through the decade of the sixties and the decades following. In 1970, 13 percent of new Ph.D.s were awarded to women; by 1989 that had increased to 37 percent. As the character of the history profession has become more diverse, so have the subjects about which historians write. Between 1958 and 1978 the proportion of doctoral dissertations written on social history quadrupled, and social history surpassed political history as the primary area of historical research.¹⁴

With these changes in the character of the nation and the historical profession it was natural therefore for historical perspectives to shift. Many historians in the 1960s and 1970s were excited by the *Annales* school and the scientific possibilities of quantification and set about writing a new kind of social history, "history from the bottom up." Instead of focusing on statesmen, generals, diplomats, and elite institutions, these new social historians, themselves often members of ethnic and racial groups newly entered in the profession, concentrated on ordinary folk and marginal people—the poor, the oppressed, the silent. Since few of these forgotten people left letters, memoirs, or the usual kind of literary documents, their stories, which tended to be group stories, had to be painstakingly extracted from the most intractable and impersonal sorts of sources—birth records, probate inventories, land titles, and tax assessments. More important than the innovative historical methods of this new social history was its significance for the traditional triumphal and exceptionalist conception of American history. The histories of outsiders, marginal people, and women could scarcely be celebrations of heroic achievements and patriotic glory; indeed, most of them were stories of frustration, despair, and defeat. The nation's history could no longer be a simple story of the triumph of liberty and democracy. In his essay Philip Gleason discusses how new accounts of immigration and ethnic interaction led historians to imagine a new and different American past and new and different ways of thinking about the country's collective identity. All the contributors to this volume are keenly aware of the profound and exciting effects the entrance of

women and new ethnic and racial groups into the profession has had on the writing of history. In the past thirty years this larger and more diverse historical profession has asked new questions of the past and has revealed a new sensibility to the historical experiences of a wide variety of hitherto neglected individuals and groups.

Dealing with large groups of ordinary people in this way increasingly led historians to imagine that there was no universal human nature that explained motivations and ascribed meanings. Consequently, they came to believe that the societies they were studying were culturally constructed, mere assemblages of meanings. This conviction in turn led in the 1980s to the adoption of a new kind of history called cultural history, which gained a widespread following among historians. Borrowing heavily from anthropology and literary theory, cultural historians tended to break up the past into discrete ethnographic moments, imagining that cultures could be studied as if they were texts, with no more than a tenuous relationship to anything outside themselves. Even Marxist historians, under the influence of theorists like Antonio Gramsci and Raymond Williams, distanced themselves from old-fashioned economic materialism and drew their inspiration from the new cultural history.

All the essays in this volume attest to the recent significance of this new social and cultural history. In its attempts to supplant the older narrative histories the new social and cultural history suggested that those narratives had been partial and propagandistic and even mythical. Some historians even ventured the thought—heretical by the standards of the profession even a quarter of a century ago—that past historical accounts were not all that different from fiction. Consequently, one of the implications of the new history was to bring the reliability and impartiality of traditional history-writing into question. As Joyce Appleby, Lynn Hunt, and Margaret Jacob point out in their book *Telling the Truth about History*, the work of the new social and cultural historians “fostered the argument that history could never be objective.”¹⁵ The women historians who were entering the profession in increasing numbers were especially attracted to the new social and cultural history; and because they were only too keenly aware of the ways in which claims of objectivity had been used to exclude them and other marginal groups from public life and indeed from history itself, they were more than willing to challenge those traditional claims of objectivity.¹⁶ Some feminist historians found in the epistemological skepticism of new literary theories a means of demonstrating the social constructiveness of gender distinctions that in turn, as Linda Kerber points out, allowed for the transformation of women’s history into the history of gender.

This emergence of epistemological skepticism is just the latest in the series of changes that have affected the writing of history during the last half of the twentieth century. But, as Dorothy Ross points out in her chapter, this “postmodern” relativism does not run deep in the profession, and because it is so destructive of all historical reconstruction it is not likely to have a lasting effect on the writing of history in the United States. What does run deep and what does seem likely to

have a profound effect on American history-writing is the fundamental transformation Americans are experiencing in their sense of national identity and their place in the world. Because the character of the nation itself is radically changing, something momentous seems to be happening to America's historical consciousness.

American historians are developing a new moral perspective, a new way of looking at the world. The distinguished medievalist and president of the American Historical Association, Caroline Walker Bynum, recently expressed some of the significance of this shift in perspective. Having been born shortly before the outbreak of the last world war, she felt that she belonged to the last American "Eurocentric generation." Consequently, "in these days of multiculturalism and postcolonial studies," she argues, "it is the task of my generation of historians to find ways of turning, responsibly and wisely, from the Eurocentric history into which we were born to the more global history our children will inherit." Bynum regards her conclusion as an "obvious insight about my scholarly generation." With Europe and America no longer at "the center of history," the American historical profession is bound to become different from what it used to be.¹⁷

American historians now seem to have a broader, more cosmopolitan sense of their country's relationship to the world. They have become involved in the world beyond their country's frontiers as never before. As Berghahn and Maier point out, American historians have become "fully integrated contributors to a broad international research community," a cosmopolitan community initially encompassing historians of North America and Western Europe, but more recently embracing Asian, African, Australian, and Latin American scholars as well. Many foreign historians now hold joint or part-time appointments in American universities, while at the same time American scholars are being invited to lecture or teach abroad. International conferences—of which the meeting in San Marino was but one example—offer new and unprecedented possibilities for scholarly communication and exchange.

Evidence that the American historical profession is becoming more international in outlook is everywhere. The works of American historians are now translated into other languages and reviewed in foreign journals with surprising rapidity. But since English has become the *lingua franca* of the contemporary world, even translations are not always necessary for the spread of American historical writings abroad. Whether in English or in translation, the works of American historians, perhaps for the first time ever, are being taken seriously by foreign scholars. American specialists in French history, for example, no longer have to wonder if French scholars are reading their works or if their books are in the *Bibliothèque Nationale*; some of them, as Baker and Zizek point out, now even find themselves equal participants in the debates with French historians over France's past. Sometimes it has even been American historians who first raised issues that transformed the history-writing of other countries. There are, for example, Eugen Weber's analysis of the social transformation of French society in the nineteenth century, Robert Paxton's studies of Vichy France, or the American

examinations of the history of the Holocaust. In short, a new modern republic of historical letters has come into being, with American historians, as a recent reviewer in *Le Monde* rather glumly noted, standing at its center.¹⁸

This broadening of perspective is changing the definition of the nation's history. Historians of colonial America no longer focus exclusively on the thirteen continental British colonies that became the United States; they now have to take account of the entire Atlantic world involving western Europe, West Africa, South America and the Caribbean, and the rest of North America.¹⁹ We are now beginning the history of American immigration from where the people came, in Europe or Africa or Asia, instead of starting the story at the docks of North America, a point of view that, as the Canadian historian J. M. Bumsted points out, "stressed, explicitly or implicitly, the unique and exceptional nature of immigration to North America."²⁰ Consequently, we now know that European and Asian emigrants went to many more places than the United States. In this new cosmopolitan atmosphere comparative studies are flourishing as never before. The history of slavery is now being viewed within the largest possible perspectives, comparing North American slavery not with just that in Latin America but, as Peter Kolchin has done, with Russian servitude as well. George Fredrickson has compared race relations in the United States and South Africa.²¹ Common circumstances and common experiences in the modern world suggest more and more comparisons between histories of the United States and other nations, involving everything from the demilitarization of economies after the world wars to the movement of people from the countryside to the cities and the development of mass politics.²² All this seems to suggest that the United States is not an exceptional place with an exceptional role in history after all.

The demise of the Soviet Union in 1989 should have left the way open for the triumphant assertion of American uniqueness and particularity. Ironically, at the very moment when their nation emerged as the world's dominant economic and military power, American historians have appeared reluctant to make such claims. American exceptionalism is losing much of its earlier resonance, and thus Europe no longer has the same meaning for Americans as it once did. The American nation does not seem to be the same either. For good or ill, the increasingly multicultural diversity of the United States is diluting and blurring an old-fashioned unified sense of American identity. Some American intellectuals are promoting a new intellectual globalism that seeks to transcend all national loyalties and even the idea of national citizenship. Some, like the philosopher Martha Nussbaum, argue for a civic education that cultivates a citizenship of all humanity, not of a particular nation. Since national identity is "a morally irrelevant characteristic," students should be taught that their "primary allegiance is to the community of human beings in the entire world."²³ With such sentiments in the air it is not surprising that some historians have difficulty holding on to traditional conceptions of the American nation.

Since the late 1960s, many political commentators, intellectuals, and historians have concluded that the United States no longer has the special, historically sanctioned role to be the path setter of humanity. The country is not exceptional

after all; it does not seem exempt from the constraints and contingencies of history. If nothing else, the conflict in Vietnam convinced many Americans that the moral character of the United States is not different from that of other nations, that they, as a people, have no specially transcendent part to play in the world. At the same time European nations have attained standards of living and degrees of freedom and democratic political stability that are equal to, if not higher than, those of the United States. Even such a conservative celebrator of America as Irving Kristol admits that America now is "a middle-aged nation," not all that different from the older nations of England and France. "American exceptionalism," he says, is virtually over. "We are now a world power, and a world power is not 'city on a hill,' a 'light unto the nations'—phrases that, with every passing year, ring more hollow."²⁴ For the first time in their history Americans are confronting the fact that the United States may be just another nation among nations without any messianic destiny. It seems just yesterday that President Kennedy told the world that "we shall pay any price, bear any burden, meet any hardship, support any friend, oppose any foe to assure the survival and success of liberty."²⁵ Today, we cannot imagine any president promising to pay any price to promote liberty in the world. To many of its own citizens, America no longer appears to be a special nation with a special destiny in the world.

It is in this context that Daniel Rodgers's chapter on the idea of exceptionalism becomes especially meaningful. For Rodgers, American exceptionalism is not mere difference. Every nation sees itself as different. Exceptionalism for him means exception from a rule, from the common tide of change, from time itself, and for Americans it has meant excepting their nation "from the universal tendencies of history, the 'normal' fate of nations, the laws of historical mechanics itself." According to Rodgers, even if the germ of the idea of exceptionalism can be traced back to the beginning of American history, most historians in the nineteenth and early twentieth centuries, including George Bancroft, Charles M. Andrews, and Charles Beard, managed to avoid being infected by it. Although Frederick Jackson Turner can be charged with the taint of exceptionalism, "the generation which launched its work in the 1940s," says Rodgers, "was the first to take exceptionalism as an American given." Indeed, "exceptionalism would not call the tune in professional historical scholarship until after the Second World War." The term itself, writes Rodgers, was coined by Stalinists in the 1920s unhappy with the heretical thinking of the American Communist Party. It then "unexpectedly found its way after the Second World War into the core vocabulary of American historical writing." Only then was an American ideology "saturated with exceptionalist convictions" able to seep into the minds of the historical profession. America's powerful Cold War culture proved capable of creating among professional historians "a desire not merely for difference but for a particularity beyond all other nations' particularities: a yearning for proof of its own uniqueness so deep that it tied every other nation's history in fetters." Only now, in the midst of profound changes in the historical profession and in America's sense of itself, suggests Rodgers, are we able to see the limitations of this exceptionalist conception of America's past.

Yet, as Rodgers notes, in the absence of this exceptionalist conception, it is hard to find a unifying historical theme for Americans. Those who would escape from exceptionalism, he suggests, have been unable or perhaps reluctant to present "an overarching conceptual framework for a nonexceptionalist history of the United States." The profession is increasingly fragmented and turning out more and more complex, technical, and specialized renditions of the past that fewer and fewer people read.²⁶ Consequently, there has been a weakening of the earlier support that the study of the past found in American society. From 1970–71 to 1985–86, years when there was a boom in student enrollments, the number of history degrees granted by all American colleges and universities declined almost by two-thirds, from 44,663 to 16,413.²⁷ The decline of the American Historical Association membership in the 1970s and 1980s is itself a sign of this weakening of the profession. The evidence compiled by Peter Novick in *That Noble Dream* reinforces the impression conveyed by these figures. He argues that the historical profession during the past several decades seems to have lost a unified sense of purpose; without a clear sense any longer of the United States's role in history the discipline seems to be coming apart. "In no other field was there such a widespread sense of disarray; in no other discipline did so many leading figures express dismay and discouragement at the current state of their realm."²⁸ Many historians see themselves as only congeries of specialists solving technical problems and talking mostly to each other.²⁹

Perhaps "the death of the past" that J. H. Plumb foresaw nearly three decades ago has come about faster than he anticipated. By "the past" Plumb meant the "created ideology," the "mythical, religious, and political interpretations" with which humans have sought to sanctify their societies, buttress their institutions, and invest their lives and their nations with a sense of destiny—interpretations, in other words, that resemble the imagined histories described in this volume. Such imagined pasts, said Plumb, should never be identified with critical history. "True history," he wrote, is basically "destructive"; "for by its very nature it dissolves those simple, structural generalizations by which our forefathers interpreted the purpose of life in historical terms." Its role is to eliminate those simple generalizations and "to cleanse the story of mankind from those deceiving visions of a purposeful past."³⁰ During the past three decades historical scholarship apparently has fulfilled its destructive role only too well, and not just in America. As Carl Schorske has pointed out, "history, conceived as a continuous nourishing tradition," no longer has the same meaning for us.³¹

Modern history-writing in the Western world, says Pierre Nora, has broken the "ancient bond of identity" with what he calls "memory," something that seems very similar to Plumb's "past." This "critical history" has ended what hitherto "we had experienced as self-evident—the equation of memory and history." History has now clearly become the enemy of memory. "History," says Nora, "is perpetually suspicious of memory, and its true mission is to suppress and destroy it."³² But of course it cannot; it just forces memory to take different forms, to become what David Lowenthal recently has called "heritage." Heritage may be a worthless sham, its credos fallacious, even perverse; but, writes Lowenthal, "heritage, no

less than history, is essential to knowing and acting.” It fosters community, identity, and continuity, and in the end makes possible history itself. “By means of it we tell ourselves who we are, where we came from, and to what we belong.” We thus tamper with our heritage, our memory, our past at our peril.³³

Be that as it may, the dynamics of historical scholarship and the demands of seeing the past as it really was can scarcely be stopped. Despite the consequent fragmentation and apparent disarray, the writing of history over the past several decades has enabled us to see more clearly than we ever could before the unusual ways we Americans have remembered the pasts not only of our own nation but of other nations as well. This volume is perhaps the best evidence of this. Probably only at this moment—when the idea of American “exceptionalism” is being seriously challenged and America’s tradition of memory has become increasingly self-conscious and historical—could this collection of papers be assembled. Just as French historians are now undertaking their own systematic investigations into the history of their profession, so too is the present an opportune moment for American historical introspection.³⁴ Only now can we fully appreciate our peculiarly American manner of conceiving of the histories of Spain, the Middle Ages, the Renaissance, or Western civilization. Only now can we understand the unusually American modes of using various theories to explain the economy, society, race, or the role of women in the past. Some, such as Carol Gluck in chapter 19, are optimistic about the recent turn of events; in her words, “with a heightened sense of historicity, a greater epistemological sensitivity, and an array of new methodological options, historians are better prepared to ask big questions without trumping them with prefabricated big answers.” Others are more cautious in their assessments of the present historiographic moment. Everyone would nonetheless agree that we may be at the end of an era.³⁵

We believe therefore that this is a particularly appropriate moment for seeing American history-writing over the past century or so in a new and fresh light. A long exceptionalist tradition in American historical thinking seems to be drawing to a close, and historians are now able to perceive with greater clarity the peculiarities of approach and interpretation that defined that tradition. This volume thus can be seen as a kind of requiem to an older provincial tradition of American historical writing.

All the scholars participating in the book were asked to reflect upon the peculiarly American approaches characterizing the study of their subjects. What did American historians in the past deem to be worthy of study? How did these preferences change over time? Is the peculiarly American historiographic perspective to be found in the subject matter, the questions asked, the approaches taken, or the assumptions brought to bear in these studies? What links did American historians make between the foreign cultures they studied and their own American culture? These are some of the issues addressed by the participants in this volume.

Although all the contributors were given the freedom to define the chronological boundaries of their essays, in most cases the authors begin their accounts at the opening of this century and carry them up to our own day. Out of these

nineteen chapters emerges a fascinating and complicated picture of America's intellectual life over the past hundred years. These wide-ranging studies of American historiography not only reveal the tensions and contrasts that existed between a learned and popular culture and between the open-minded and provincial members of the historical profession, but they bring to light the Americans' often intense struggle to accommodate their study of the past to the needs of their present. In the end this collection of essays tells us as much about America as about American history-writing.

NOTES

1. For similar sense of a historiographical revolution in France see the recently published *L'histoire et le métier d'historien en France 1945–1995*, sous la direction de François Bédarida (Paris, 1995).

2. Robert O. Simmons and Dolores H. Thurgood, eds., *Summary Report 1994: Doctorate Recipients from United States Universities*, National Research Council (Washington, D.C., 1995), 49.

3. Byron Shafer, ed., *Is America Different? A New Look at American Exceptionalism* (Oxford, 1991); Seymour Martin Lipset, *American Exceptionalism: A Double-Edged Sword* (New York, 1996). For criticism of American exceptionalism see Ian Tyrrell, "American Exceptionalism in an Age of International History," with a critique by Michael McGerr and a rejoinder by Tyrrell, *American Historical Review* 96 (1991): 1031–72. For a sympathetic view of America's uniqueness see Michael Kammen, "The Problem of American Exceptionalism: A Reconsideration," *American Quarterly* 45 (1993): 1–43.

4. John Higham, "The Future of American History" *Journal of American History* 80 (1994): 1289–1309; Kammen, "American Exceptionalism"; Jack P. Greene, *The Intellectual Construction of America: Exceptionalism and Identity From 1492 to 1800* (Chapel Hill, 1993).

5. Greene, *Intellectual Construction of America*, 4, 6.

6. Freneau and Brackenridge, "The Rising Glory of America" (1772), in Fred Lewis Pattee, ed., *The Poems of Philip Freneau: Poet of the American Revolution* (Princeton, 1902), I, 74, 78.

7. Kenneth Silverman, *A Cultural History of the American Revolution: Painting, Music, Literature, and the Theater in the Colonies and the United States from the Treaty of Paris to the Inauguration of George Washington, 1763–1789* (New York, 1976), 9–11, 228–35.

8. Quoted in Gilbert Allardyce, "The Rise and Fall of the Western Civilization Course," *American Historical Review* 87 (1982): 709.

9. Charles H. Haskins, "European History and American Scholarship," *American Historical Review* 87 (1923): 215–27.

10. Not until well after World War II did American historians of the Renaissance begin to emphasize the negative valences in Burckhardt's concept of modernity. Until recently then this concept provided a bridge between fifteenth-century Italy and modern America.

11. Felix Gilbert, *To the Farewell Address: Ideas of Early American Foreign Policy* (Princeton, 1961).

12. "Theory and Practice in Historical Study: A Report of the Committee on Historiography," *Social Science Research Council Bulletin* 54 (New York, 1946); "The Social Sciences in Historical Study—a Report of the Committee on Historiography," *Social Science Research Council Bulletin* 64 (New York, 1954). An excellent survey of this question is found in

Lawrence Stone, "History and the Social Sciences in the Twentieth Century," in Charles F. Dalzell, ed., *The Future of History: Essays in the Vanderbilt University Centennial Symposium* (Nashville, 1977).

13. R. Bendix, *Max Weber—An Intellectual Portrait* (New York, 1960), xx–xxiii. See also the "Bibliography on Max Weber" in *Social Research* 16 (1949): 70–89.

14. Joyce Appleby, Lynn Hunt, and Margaret Jacobs, *Telling the Truth about History* (New York, 1994), 147–48.

15. Appleby, Hunt, and Jacob, *Telling the Truth about History*, 200.

16. Appleby, Hunt, and Jacob, *Telling the Truth about History*, 2.

17. Caroline Walker Bynum, "The Last Eurocentric Generation," *Perspectives: American Historical Association Newsletter* 34, 2 (Feb. 1996): 3–4.

18. Nicolas Weill, "L'histoire s'est arrêtée à Montréal," *Le Monde*, 8 Sept. 1995, viii: "Au Québec, et quarante ans plus tard, l'histoire sociale existe encore, à côté d'autres écoles. Montréal marquera-t-il aussi une consécration? Si tel est le cas, ce sera, à n'en pas douter, celle de ce que l'on qualifiera peut-être, en 2035, d' 'école américaine.' " . . . These are the concluding sentences of a long article devoted to the Congress of Historical Studies held in Montreal at the end of August 1995.

19. Bernard Bailyn, "The Idea of Atlantic History," *Itinerario* 20 (1996): 1–27.

20. J. M. Bumsted, Book Review, *Journal of American History* 82 (1995): 1181.

21. Peter Kolchin, *Unfree Labor: American Slavery and Russian Serfdom* (Cambridge, Mass., 1987); George Fredrickson, *White Supremacy: A Comparative Study of American and South African History* (New York, 1981); Fredrickson, *Black Liberation: A Comparative History of Black Ideologies in the United States and South Africa* (New York, 1995). For another recent comparative study see Colleen Dunlavy, *Politics and Industrialization: Early Railroads in the United States and Prussia* (Princeton, 1994).

22. Of course, comparative history may not lead at all to less emphasis on America's exceptionalism. As Marc Bloch pointed out long ago, and Michael Kammen and others have recently underlined, people often suppose "that the [comparative] method has no other purpose than hunting out resemblances." "On the contrary," said Bloch, "correctly understood, the primary interest of the comparative method is . . . the observations of differences." Quoted in Kammen, "American Exceptionalism," 19. This has been borne out by recent comparative studies, which often begin by emphasizing America's similarity to other cultures but usually end in highlighting its differences. American historians, for example, working in comparative race relations, have eventually come to appreciate the peculiarity of the American theory of race—"that each person has a race but only one." Gary B. Nash, "The Hidden History of Mestizo America," *Journal of American History* 82 (1995): 960.

23. Nussbaum, quoted in Michael J. Sandel, *Democracy's Discontent: America in Search of a Public Philosophy* (Cambridge, Mass., 1996), 341.

24. Irving Kristol, "America Dreaming," *On the Issues: American Enterprise Institute* (Sept. 1995).

25. Quoted in James T. Patterson, *Grand Expectations: The United States, 1945–1974* (New York, 1996), 486.

26. Paula Baker, "The Fragmentation of the Profession and Its Class Culture," *Journal of American History* 81 (1994): 1148–49.

27. C. Vann Woodward, *The Future of the Past* (New York, 1989), 26–27.

28. Peter Novick, *That Noble Dream: The "Objectivity Question" and the American Historical Profession* (Cambridge, 1988), 578.

29. David Thelen, "The Practice of American History," *Journal of American History* 81

(1994), 933–60; Thomas Bender, “‘Venturesome and Cautious’: American History in the 1990s,” *Journal of American History* 80 (1994): 997.

30. J. H. Plumb, *The Death of the Past* (Boston, 1970), 11–17.

31. Carl Schorske, *Fin-de-Siecle Vienna: Politics and Culture* (New York, 1980), xvii.

32. Pierre Nora, “Between Memory and History: Les Lieux de Mémoire,” in Jacques Revel and Lynn Hunt, eds., *Histoires: French Constructions of the Past* (New York, 1995), 632–36. For nearly a decade Nora has been assembling seven volumes of *Les lieux de mémoire*, which was designed, said Nora, to “decompose the unity” of the French nation. A first volume of an English translation was recently published with the title *Realms of Memory: The Construction of the French Past* (New York, 1996).

33. David Lowenthal, *Possessed by the Past: The Heritage Crusade and the Spoils of History* (New York, 1996), ix–xiii.

34. See the papers in the *American Historical Review* 100, 2 and 3 (1995), which attempt, as do the essays in this volume, to describe the peculiar ways Americans have written about the pasts of themselves and of other peoples.

35. It is only this sense of being at the end of an era in American history that makes possible a book like that of the late Dutch historian Jan Willen Schulte Nordholt, *The Myth of the West: America as the Last Empire* (Grand Rapids, Mich., 1995).

Exceptionalism

DANIEL T. RODGERS

“Is AMERICA different?” a recent collection of scholarly essays undertook to inquire.¹ One might have thought it a puzzling question, so rhetorically framed, the implied comparison so remarkably vague, and yet the answer, whatever the comparative yardstick should turn out to be, so patently “yes.” And, indeed, “yes” even if another subject were to be introduced in the place of “America,” so that the question came to read “Is Argentina different?” or Afghanistan. One might have thought it a remarkable question, that is, had it not been asked so often before in the American past and with such intense hunger for an affirmative answer.

To the question “Is America different?” the professional historian is expected to respond with a list of the circumstances and exemptions which have distinguished the history of the United States: a land radically underpopulated by European standards and (given the vast inequalities in biological immunities and technological inheritances between its inhabitants and its invaders) relatively costlessly conquered, abundant in readily exploitable natural resources, far from the great powers and the central cockpit of great-power warfare, without an hereditary aristocracy monopolizing landed property and the offices of the state and therefore (in contrast to the *anciens régimes* of Europe) relatively costlessly democratized, all in a period of modern world history when economic growth came relatively easily to nations close to the western European core, and so on. All (and more) are true, and all important.

But if the answers matter, so at a deeper level does the inquiry itself. It was an odd question, so timelessly phrased, to have lodged itself in a discipline in which possession of the Ph.D. putatively certified an expert sensitivity to time and mutability. At the height of the Cold War, American “exceptionalist” history bore, through a curious kind of political cross-dressing, a Stalinist neologism for a name. Of the controlling themes in contemporary United States history writing, however, none were pressed more urgently upon professional historians by the surrounding culture than a desire not merely for difference but for a particularity beyond all other nations’ particularities: a yearning for proof of its own uniqueness so deep that it tied every other nation’s history in fetters. What was the historiographical past of that conceit? What are its current historiographical tendencies?

In a debate in which substance so often slides into wordplay, let us start with the terms themselves. Whatever the argument about American differentness may have been about, it was not about difference in itself. In difference, pure and simple, nothing special or distinguishing is to be found. "All nations are different," Joyce Appleby puts the common sense of the matter in a recent essay; "and almost all national sentiments exploit those differences."² Both in mind and in fact, uniqueness is every nation's lot. Around so bare a tautology, the rhetorical and analytical heat sufficient to kindle an historiographical controversy could never have been collected.

In the skein of tropes which, from the seventeenth century forward, Euro-Americans wove around that part of the Americas which became the United States, difference, in fact, was only one of several strands, and neither the most distinctive nor the most important. The New World's "newness" was a more striking metaphor. First used to represent the Europeans' surprise at the unexpected inaccuracy of their geographic knowledge of the world, it became, in the revolutionary atmosphere of the 1770s and 1780s, the marker for a claim much headier still, the opening in British North America of an era in social and political relations unknown to history before: a "new order of the ages."³ Yet a third thread has been the providential one. From John Winthrop to Oliver North, the "rituals of God's country" (as Sacvan Bercovitch once called them) have run: a sense of God acting within and through the history of His specially chosen land.⁴ Not the least, difference in American national culture has meant "better": the superiority of the American way. The distinctions merge and tangle. To be different is to be best, to claim membership in a new and uniquely blessed society.

Little in this bundle of sentiments, however, is as peculiar to the United States as Americans tend to imagine. If the Americans are "different," the British have their treasured peculiarities, the French their *génie français*, nineteenth-century Germans their distinctive *Kultur*. Americans have not been the only people to imagine restarting the clock of time or to know the euphoria of thinking, in Thomas Paine's words, that the "birthday of a new world" was at hand. Pride and providentialism are too widely spread to imagine them American peculiarities. If European visitors to the nineteenth-century United States suffered under the self-importance of the Americans, American travelers to Wilhelmine Germany and late-Victorian England suffered under a sense of national superiority no less overweening and self-satisfied. Throughout history armies have assembled with God as their marshal, and statesmen have plotted foreign policy as if they were agents of Destiny itself. These are not accidental similarities. Cultivation of sentiments of difference and superiority has been, from the early modern era to the postcolonial present, at the heart of the project of nation-state formation. To make a peasant think like a Frenchman (or an Indian or Iraqi) nothing matters more than clapping on that citizen's head a sense of national uniqueness.

Within these common terms, however, there has run a thread which, if not wholly distinct to the American complex, carries there a peculiarly striking weight. That is the idea of "exceptionalism." Exceptionalism differs from difference. Difference requires contrast; exceptionalism requires a rule. Difference

claims feed on polarities and diversity; exceptionalist claims pin one's own nation's distinctiveness to every other people's sameness—to general laws and conditions governing everything but the special case at hand. Exceptionalism, Appleby writes, posits "deliverance from a common lot. There are no exceptions without well-understood generalizations or norms in contrast to which the exception commands attention."⁵ When difference is put in exceptionalist terms, in short, the referent is universalized. Different from what? Different from the universal tendencies of history, the "normal" fate of nations, the laws of historical mechanics itself.

The term "exceptionalism" was a latecomer to American historical and political analysis, a Stalinist coinage of the 1920s which unexpectedly found its way after the Second World War into the core vocabulary of American historical writing. The term was bound up from the first in what Marx himself called the general "laws of historical motion"—the world historical movement toward ever more intense forms of capital accumulation, immiseration, proletarianization, and class conflict which would ultimately bring down the house of capital. A theoretical issue for turn-of-the-century Marxian intellectuals—not the least with regard to the United States, whose development was not always easy to fit into the general line—the idea of universal history had become by the late 1920s a club to beat deviant national communist parties into submission. Among those was the American Communist Party of the late 1920s. Overwhelmed by the difficulties of organizing in the era of Henry Ford and Herbert Hoover, its leaders resisted the Comintern's pronouncement that a "third period" in modern world history had begun in early 1929, insisting that the American economy, not yet having reached the point of "collapsing stabilization," remained stuck in capitalism's upward phase. Worse, they asked for greater autonomy in day-to-day tactics and strategy. Summoned to Moscow for a laboriously contrived airing of the "American" question, the American party leaders made the mistake of linking their cause to that of Nikolai Bukharin just as Stalin was arranging his coleader's deposition. Branded with the heresy of "exceptionalism," the Americans were ejected from the party and a rival cadre installed in their place. Stalin, who engineered the event, had no interest in the historical issues at stake. The ejected Lovestoneites, for their part, had neither used the term "exceptionalism" nor claimed that American economic developments were of a different order than economic developments elsewhere—only that they lagged behind.⁶

From such profoundly inauspicious beginnings, the idea of exceptionalism might have been expected to die a quick conceptual death. Had it not been for a set of powerful homologies with other currents in the national culture, it could hardly have slipped the peculiar circumstances of its birth. In other keys and other registers, however, exceptionalist history had roots in America deeper and firmer than Marxism. Protestant Americans had long had their own universal history, written in the language of eschatology and millennialism, their own basic law of sinward historical motion from which a special people might be chosen out, a nation "elected." "Republican" versions of exceptionalist history secularized the terms of Protestant history but kept the exceptionalist structure intact.

Nations rose in bursts of power and fortune, but the general drag of history was downward as morals and manners decayed under the temptations of private wealth and self-regarding egoism. Let the general tendencies of time go unresisted, and the normalization, the "Europeanization," of the American republic was its expected fate—unless, through virtue, a nation should hoist itself free of the common tide of change, elect itself out of time.⁷ Rule and exception: into the pattern which smelled so heretical to the American commission in Moscow, earlier and later variations on the exceptionalist theme have run, homogenizing in the very act of differentiating, laying down universal law in the very act of claiming, but only for the United States, a special escape clause.

In all these formulations, it has never been easy to distinguish where analysis halted and exhortation began. From John Winthrop's assertion forward—not that God had set the Puritan project "upon a hill" but that his company, quarreling in mid Atlantic, should conduct themselves in hope that He might—the conditional and the factual in exceptionalist rhetoric have been exceedingly difficult to pry apart. In the United States, however, exceptionalism has drawn on wells of pride as deep as those of anxiety. The especially exempt nation might default on its freedom from all other nations' lot, but the terms of the proposition were not widely doubted. Exceptionalist rhetoric in the United States differs in that regard from the exceptionalism of most of the other nations—materially so similar—formed on the edges of the early modern European empires. There, too, one finds strong assumptions of normal national development. But in Canada and Latin America, observers stress, the rhetoric of exceptionalism is one of estrangement from the normal, of absence, loss, and regret. They are lands on the margins, their identities still partly colonial, their mythology, as Sacvan Bercovitch writes of Canada, "elsewhere."⁸

In the United States, by contrast, exceptionalism is accepted not as a deficit but as a gift. It, too, is a rhetoric of absences, but the absences are the ills and defects of a universalized external world. Still, having posited its identity in difference, and its difference in exemption from the rule, the American myth lies, in its own way, "elsewhere." A nation which conceives of itself in exceptionalist terms is fated to spend at least as much of its popular historical energy imagining everyone else's history as in writing its own. That is one of the unrecognized ironies of American history.

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Though they wrote within a national culture steeped in exceptionalist convictions, the historians who shaped the professional writing of American history in its formative years did not always keep in step. Inhabiting scholarly worlds which were as Atlantic as they were American, they were better trained to see the history of the United States as proceeding within world history than outside it. Their America answered less often to historical necessities all its own than to broader, transcultural historical forces: the progress of nations, the rise of Europe or the West, or the updraft of civilization itself. Arguments coming in family clusters,

exceptionalism entailed in its very frame its antitheses and alternatives: connectiveness, complicity, embeddedness. As the historical profession gathered itself together in the 1880s and 1890s, it would be fair to say that the latter had the upper hand.⁹

These took a number of different forms. In George Bancroft's *History of the United States*, brought to completion in 1882, the prime mover was the "world spirit" of liberty taking up, for a moment, its abode in America. That was what made the American Revolution not merely a national affair for Bancroft but "an assertion of rights . . . for the entire world of mankind and all coming generations, without any exceptions whatsoever."¹⁰ By the end of the nineteenth century, Bancroft's faith in the "unity of mankind" had worn thin, replaced in the Eastern gentry social circles, from which the new university faculty were being heavily recruited, by a much more cramped and constricted belief in something they alternatively called the Teutonic or the Anglo-Saxon heritage. A handy device to extract the essence of American history from the possibility that immigrant and nonwhite Americans might lay serious claim to it, Anglo-Saxon historiography nonetheless made no illusions of national self-sufficiency. "No nation has a history disconnected from that of the rest of the world," Harvard's Albert Bushnell Hart began his list of the "fundamental principles of American history" in 1883. "Our institutions are Teutonic in origin; they have come to us through English institutions."¹¹

The heaviest opposing gun on the exceptionalist side was Frederick Jackson Turner's. In his emphasis on "perennial rebirth" as the defining American experience and his explicit sense of deviation from the norms of social development which held sway everywhere else, Turner gathered the tropes of exceptionalist history into particularly powerful form. The spatial vagueness of Turner's frontier has often been noted, for it was never so much a place on the map as the site of Europe's negation. Arriving on the margin of settlement "European in dress, industries, tools, modes of travel, and thought," Turner wrote in one of the most famous passages of his 1893 essay, the pioneer fell out of history's normally irreversible necessities to be rebaptized as an American. No less than Marx, Turner had his laws of historical motion: from simple to complex, from primitive to manufacturing economies, from savagery to civilization, all the usual social-evolutionary baggage of a well-educated late Victorian. But the frontier was America's emancipator; there the general laws of historical motion were turned back. The frontier was America's "gate of escape from the bondage of the past," Turner wrote: from the gentry-historians' Teutonic germs, from Europe, indeed from history itself.¹²

How so metaphoric an essay as Turner's "The Significance of the Frontier in American History" should have established itself as the most important single piece of historical writing to come out of the late nineteenth-century United States can scarcely be comprehended outside the popular convictions Turner's word pictures caught so well. Pitching camp at the intersection of academic scholarship and Chautauqua culture, Turner's frontier thesis became, in truth, all

but immobilized there. Turner's was a boundary-condition form of exceptionalism, written in full, nervous awareness that the material conditions he held responsible for everything exceptional about the United States were now exhausted. If the frontier held the exceptionalist key Turner posited for it, there was no way, with the frontier's passing, to prevent the New World from growing old. But the line between history and faith has always been thin in the exceptionalist camp, and never thinner than with Turner. For the next twenty years in, the face of his own environmental materialism, Turner could not resist exhorting his countrymen to make their "stand against the tendencies to adjust to a European type" by an act of will alone.¹³

For all of Turner's prominence, however, exceptionalism did not yet call the tune in professional historical scholarship. The biggest and most ambitious collective work of historical scholarship of its time, Harper and Brothers's twenty-eight volume history of the United States, published between 1904 and 1918, opened not with the frontier but with William Cheyney's *European Background of American History*, followed, remarkably enough, by Livingston Farrand's detailed ethnographic account of the American Indian peoples, before settling into what American historians of the 1940s and 1950s were to think of as the beginnings of American history proper, the English settlement of the Atlantic seaboard.¹⁴ Under the leadership of Charles M. Andrews, the most authoritative students of the American Revolution withdrew the inflated patriotism from that event to redescribe it as a series of misunderstandings within an Atlantic, English-speaking empire.¹⁵ As for the giants of early twentieth-century American history writing—Charles Beard and W. E. B. Du Bois—they had absorbed too much of class and economic analysis and instinctually thought on too large, world canvases to be exceptionalists. "The discovery, settlement, and expansion of America form merely one phase in the long and restless movement of mankind on the surface of the earth," Charles and Mary Beard began the most influential American history book of the early twentieth century. The twist of the knife came, of course, in that gratuitous word "merely." In the Beards' *Rise of American Civilization* (1927), economic processes heaved and strained, empires rose, and interests clashed, but the hand of special destiny was not in American history.¹⁶

Writing in a culture saturated with exceptionalist convictions, in short, professional historians did not unquestionably swallow the exceptionalist premises. They did not because they were Hegelians (like Bancroft), or elitists (like Hart), because they knew worlds far beyond America (Oxford radicalism for Beard, student Berlin for Du Bois), or because (like Andrews, burrowing deep in English sources) the facts did not seem to add up for them in exceptionalist patterns.

It was the second shattering of Europe in the late 1930s, the "suicide" of the Old World, as many Americans were calling it in the 1940s, that changed the historiographic mood. The generation which launched its work in the 1940s was the first to take exceptionalism as an American given. In retrospect it seems clear that they did so, not because the phenomena they stressed in the American past had gone unnoticed before, but because their understanding of Europe had been so dramatically shaken. One had to read between the lines to see it, but what was

being rewritten was not so much the special history of the United States but, in a newly tragic key, the general laws of historical motion themselves.

Among the several telltale marks of the new historiographical turn a particularly striking one was the sudden ubiquity of references to that failed eighteenth-century French expatriate and travel writer, J. Hector St. John de Crèvecoeur. Virtually unread in the United States before the twentieth century, Crèvecoeur's lyric passage on the transforming effects of American society on its immigrant newcomers, its "melting" of persons of all nations into "a new race of men," extracted from context, retitled as "What Is the American, This New Man?" now seemed to appear everywhere.¹⁷ Arthur Schlesinger, Sr., made it the motif of his presidential address to the American Historical Association in 1942.¹⁸ The literature of the new American Studies movement was saturated with Crèvecoeur references. They led off that catalyst for revisionist histories of the Revolution, Robert E. Brown's *Middle-Class Democracy and the Revolution in Massachusetts*, in 1955.¹⁹ It was scarcely possible to write about the relationship between American and European history without them. That so liminal a figure as Crèvecoeur, neither of France nor of British North America but stranded in between by a Revolution whose rationale he professed himself ignorant, whose "innovations" he distrusted, and whose violence he detested,²⁰ should have come to hold the key to the land from which, in 1780, he had fled suggests how deep the historians' stake in an America broken free of Europe's past had suddenly become.

The Crèvecoeur revival was a surface symptom of deeper shifts in intellectual foundations. From the wartime morale projects, studies of national character and personality moved into the forefront of postwar social science. References to the "American national character" sprouted in the "soft" multidisciplinary eclecticism of the new American Studies programs and in the "hard" social sciences alike.²¹ David Riesman subtitled his *Lonely Crowd* in 1950 a study in "the changing American character." David Potter, in a revision and extension of the Turnerian frame, used it to suggest abundance as the key to the special history of the United States.²² Through the work of Perry Miller and others, the providential strain in the American past was drawn into the center of historical scholarship. Extracting New England Puritanism from the aspersions into which the prohibition-baiters of the 1920s had thrown it—as the sin-obsessed, pleasure-denying shadow on the American psyche—Miller and his students held up New England separatist utopianism as the key to the national mind and culture. John Winthrop's "city upon a hill," beacon to a world whose fate it had escaped, was no longer a mid-Atlantic hope, or even Boston; it was now America itself.²³

The need to articulate a distinctive "American way" for the war and Cold War played a central role in these exceptionalist projects. But a less overtly political necessity drove them as well: the need, in the face of traditional Europe's collapse, to explain how it had come about that the United States seemed to have skated unscathed past the disasters of the mid twentieth century, past the revolutions of the left in Russia and of the right in Italy, Germany, and Spain, the inner collapse of 1940s France, the bankruptcy of Britain, the perilous instabilities of central and southern Europe. If these were the tendencies of history—a Marxian

world of upheaval and revolution, of sustained and brutal class conflicts but stripped of Marx's illusions about their happy outcome—then the history of the United States was truly exceptional.

In this rethinking of the general tendencies of history, the American Revolution occupied a specially important place. Where Andrews had told a tale of an empire torn apart by miscalculation, where the Beards had seen economic universals at work, and where Crane Brinton had discerned the general shape of revolution itself,²⁴ the postwar historians of the American Revolution reformulated the event as an extraordinary historical anomaly. In a world haunted by the destabilizing effects of revolution, the American case seemed *sui generis*: a popular revolution which had remained within moderate, Lockean bounds, a revolution without its Jacobins or Bolsheviks, without its reign of terror, without its Lenin or Robespierre. A European historian like Robert Palmer might write of a general age of eighteenth-century revolution,²⁵ but for American historians the upheavals of 1776 and 1789 were, symbolically and historically, worlds apart. The American Revolution was "anything but revolutionary," Robert Brown wrote with a piece of his mind, like that of so many of his fellow historians, on Paris. It was "unique," as he tried to show from the distribution of property in the Massachusetts colony on the eve of independence, because social stratification of the North American colonies was, itself, uniquely foreshortened into a structure "almost the exact opposite of European society."²⁶

Starting there, the rest of American politics unfolded. Where the Revolution was reconceptualized as an extraordinary break in the normal laws of revolution, the subsequent course of politics was remade as a triumph of classlessness. Only after the radical fires of the 1930s had cooled, only with the incorporation of the CIO into the coprosperity spirit of postwar economic growth, and, most importantly, only against the background of Europe's catastrophe could such a reading of American politics have taken hold. Arthur Schlesinger, Jr.'s, *The Age of Jackson* (1945) still breathed the spirit of prewar history writing, but that spirit was rapidly thinning out.²⁷ Extracting "exceptionalism" from Communist Party jargon, scholars moving centerward from the anti-Stalinist left injected it into the central vocabulary of American social and political science. An absence—the relative failure of socialism in the United States—became the defining point of the nation's history, a ratification of the special dispensation of the United States in a revolutionary world where Marx still tempted.

No book brought together the themes of postwar exceptionalist history more influentially than Louis Hartz's *The Liberal Tradition in America* in 1955. Hartz, to be sure, was a student of political theory not an historian. Beyond early nineteenth-century Pennsylvania, whose political economy he knew intimately, his history was never more than schematic; but he put in vivid form what postwar Americans wanted to hear: that the laws of historical motion which held Europe by the throat did not run in the United States. Like Turner's, Hartz's was a boundary condition form of exceptionalism. Starting without a feudal past, a bourgeois "fragment" of European society, American history could not but unfold differ-

ently. But unlike Turner's exceptionalism, in which the general forces of history retained their sway behind the moving frontier, in Hartz's exceptionalism time itself in America came to a halt. Without a feudal past, the inner, dialectical engine of history had no purchase. No Robespierre, no de Maistre, no Marx, no Goebbels, no Stalin, only (in the shorthand Hartz affected) an eternal Locke. Other nations went through the throes of the twice-born, but the Americans, by the chance conditions of their founding, had slipped free of the underlying motor of historical change. Starting differently, they were fated to be eternally the same—and eternally different from everyone else.²⁸

The "storybook truth about America," as Hartz called it, was that of a people who, in escaping Europe's past, had found their own exceptional future, freer and yet more stable than Europe's. So self-evident did that tale seem that the historians who contributed to it would have found it hard to recognize how strikingly, in their anti-Marxism, they had reimagined Marx's general laws of historical motion applied everywhere but to their own national case. The revolutionary instability which, projected elsewhere, seethed between the lines of United States history writing had hardly been absent, of course, from the history of the United States itself. In another setting, the combustible elements of race and slavery, the thirty-year struggle over secession, and the cataclysm of the Civil War might have rattled the assumptions at the core of exceptionalist history. But where the Paris Commune was France, the Civil War, though an incomparably larger historical event, was somehow beside the point which was America. The exceptionalist historians' hands might be deep in the archives in America, but a part of their mind was fixed elsewhere: on Paris in 1793 with its guillotine working at fever pitch, Leningrad as the Winter Palace was stormed in 1917, or Berlin with its streets full of brownshirts in 1933. It was only against this selective history of Europe, amalgamating other nations' histories into a single theme that proved the distinctiveness of their own, that the American past seemed stable, "seamless" (as Daniel Boorstin called it), an exemption from the rule.²⁹

Exceptionalist American history, even at its height, never controlled the entire terrain any more than had the cosmopolitan history before it. Courses in Western Civilization, designed to glue the aspirations of the American present to the best of the European past, proliferated in the 1940s and 1950s—though historians of the United States rarely taught in them, not merely because they had other work to do but because the conceptual moat remained too large.³⁰ Other historians sounded the call for American leadership of the Atlantic community or the "free world." But nothing showed the massive presence of exceptionalist assumptions more than the difficulty critical historians in the 1960s and 1970s had in surmounting them. The "new" diplomatic historians trained in William A. Williams's workshop recovered an empire which had been all but suppressed from political memory, but only to explain it as a projection of uniquely American forces and values.³¹ A new generation of immigration historians showed assimilation to be much more difficult and incomplete than regenerative or melting pot accounts had had it; but starting their stories where exceptionalist histories

had begun them, with the immigrants' moment of entrance to the New World, they found it far easier to qualify the master trope of acculturation than to overturn it.³²

The new social historians, setting out to show the hollowness of the core premises of exceptionalist history—to demonstrate empirically that social mobility in the United States had never been as far-reaching as the rags-to-riches myth had supposed, that the early democratization of American politics had failed to penetrate beyond a privileged circle of white male citizens, that class and racial divisions ran too deep in the United States's past to ignore, that the American working class had not played passively into the emerging capitalist and wage-labor regime but had resisted it, more violently, indeed, than the working classes in Europe—even they did not ultimately escape exceptionalist history's structures. The vigor of this recovery of an authentic history of working people in the United States, slave and free, immigrant and American-born, female and male, can hardly be exaggerated. But though these historians dramatically complicated the story of class relations in the United States, they did not escape the question the exceptionalists had made the central one: why normative, European-style socialism sank such shallow roots in the United States. As long as the only effective negation of exceptionalist history was to find a radical tradition, an autonomous working-class culture conscious of its own self and political interests, equal in historical centrality to the prewar German Social Democrats or the postwar British Laborites, the dissenting historians' quest was doomed to frustration. The narrative thread of the new social history unrolled as a string of heroic failures and frustrated opportunities. Its bottom line was not fundamentally different from Hartz's; there was not enough socialism in America after all.³³

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To challenge the exceptionalist frame of postwar American history writing would take more fundamental recasting of the image of world history in the American historians' minds' eye. Within the last decade, however, it is clear that something of that sort has begun to appear across a broad historiographic front. Albeit dogged with controversy, a postexceptionalist American history has come into view.³⁴

One of the key events in this regard has been the fading away of the exceptionalists' imagined Europe and, with it, the imagined rules of other nations' histories. Fifty years after the end of the Second World War, the inherent instability of western European society no longer seems an historical given. With Communist Europe in disarray, the teleological engine of Marxist history has likewise broken down. Neither Marx's conviction that the common forces of capitalism would strip the proletariat "of every trace of national character" nor the alternative, liberal theories of convergence on a common modernity command assent. Throughout the modern world, economic integration and national particularization both proceed at once. The imagined central tendencies of history no longer hold. But in a world without rules there can be no exceptions—only an infinite regress of differences.

The fading of the teleological arrow of exceptionalist history has had its most striking impact on American labor history. Sean Wilentz's "Against Exceptionalism," roughly handled a decade ago, now speaks the field's conventional wisdom. No historic model of working-class organization, it is now clear—not German social democracy, not French radical syndicalism, not Scandinavian farmer-labor welfarism, nor British Lib-Laborism—captured that Hegelian will-o'-the-wisp: consciousness of class in and for itself. There have been virtually as many socialisms as there have been European nations.³⁵ Acceptance of a world without a normative path of working-class development has freed American labor historians to focus less on absences than on presences in the American past. It is now possible to write about the Populist movement and the Knights of Labor without apologizing for their insufficient socialism. The old structural arguments for the failure of working-class politics in the United States—the lure of cheap land and high wages, the ethnically fractionated character of the labor force and the racialization of its inner identities, the early incorporation of white working-class males into electoral politics, the antistatist animus in the political culture at large, and the particularly inhibiting constraints of two-party, majoritarian politics—have not disappeared. But one now hears less about permanent structural deterrents and more about contingency and history, about particular, fortuitous convergences of forces and events.³⁶ The return of contingent history does not yield a working class in the United States like that of other working classes. But the question of difference was, from the beginning, false and tautological. The antithesis at the core of exceptionalist history was never that between difference and sameness but between autonomy and connection.

In that sense, the less heralded aspect of the emerging historiographic revolution has been more far-reaching: a recognition of American complicity in larger world historical forces. Abandonment of dichotomous for connected development has gone farthest in the writing of colonial American history. In a recent historiographical overview, Joyce Appleby has stressed the colonial social historians' embrace of the techniques of the *Annales* school and the Cambridge demography group as the moment when American historians rediscovered the Europeanness of colonial America. Immerse oneself in the early records of a New England town, Kenneth Lockridge had reported in 1970, and one found not the American "national character" but a strain of that same peasant culture, wedded to "ancient, universal patterns of rural life," which permeated early modern Europe.³⁷ But in retrospect the more transformative event was not the new techniques—which just as often skittered away from Lockridge's conclusion—but the rediscovery of the Atlantic economy. A key moment in this regard was Philip Curtin's census of the Atlantic slave trade. In the efflorescence of comparative slavery studies Curtin's work helped to fuel, it was possible to read an exceptionalist moral for the American case. Only in British continental North America did slave populations sustain themselves demographically against the economic advantages which ran everywhere else toward extremely high ratios of male to female slaves: toward a familyless labor force of young, expendable, repurchasable human resources. But the larger implication of Curtin's work was to bring

back into focus the extraordinary dimensions of the Atlantic trade in human bodies between Europe, Africa, and the Americas. If slavery was central to American history, as a spate of books was arguing eloquently and irrefutably by the 1970s, then the forces of the Atlantic economy were inextricable from its core dynamics.³⁸

Where Curtin filled in the Atlantic with enslaved Africans, others began to fill in the seventeenth-century and eighteenth-century Atlantic in other ways. Alfred Crosby revealed an Atlantic filled with plants, food crops, and pathogens—not a barrier at all but a broad highway of biological exchange. Historians of American Indian–European relations refound a world trade in furs whose fingers reached, with profoundly destabilizing effects, deep into the continent. Bernard Bailyn filled the Atlantic with migrants and land speculators, David Hackett Fischer with folkways in transit, Richard Bushman with the material goods of fashion. Even the British empire has begun to turn up again as an object of historical inquiry.³⁹

This repositioning of colonial British America at the western rim of a vast Atlantic economy was abetted by the movement of the center of interest in colonial American history out of New England—where Miller, Bailyn, and Edmund Morgan had helped fix it in the 1950s—to the Chesapeake. That movement, visible by the mid 1970s, had as much to do with the diminishing returns of an overcrowded historiography as any broader agenda. But it soon became clear that reintegration of the South into the core American story meant substituting for stories of boundedness (as in Lockridge’s “closed, corporate community”) stories of instability and penetration, in a region as porous as its estuaries were to the sea itself. The contrast to the New England studies, framed within localistic, Durkheimian narratives of the accretion and release of social strain, was sharp and far-reaching. Tobacco, slaves, servants, goods, elites, and ideas passed too quickly through the Chesapeake settlements to make even the very boundaries of an American history distinct.⁴⁰

The colonial British America which emerged from these studies remained profoundly different from Britain itself; but it was the difference of a periphery to its center, the extractive edge of a commercial empire to its core. In a world of unequal and specialized distribution of functions and labor, connection proves, in fact, to be a far more powerful explanation for difference than mere distance. As the metaphors of isolation melt away, it has begun to be possible to see on the western edge of the Atlantic not “America” but British (and Spanish) North America, embedded in larger imperial projects and world systems of commerce, labor, and power.

This notion of an Atlantic system has not yet transformed the history of the Revolution as profoundly as the period preceding it, but here, too, the effects of a changed point of view can no longer be dismissed. Whatever causes may be adduced for the breakup of the empire, it is clearly no longer possible to describe it as the estrangement of two increasingly dissimilar peoples. From the work of Bailyn, Gordon Wood, Jack Greene, and many others, it is now clear that when it came to mobilizing opposition to British colonial policy—to putting words on

the colonists' opposition to taxation and trade restrictions, to an occupying and expensive army, to the usual concomitants of imperial administration—the patriots borrowed heavily from the political language of British radical Whiggism and the precedents of British parliamentarianism. Knowing London better than they knew each other (as Garry Wills notes in a striking passage in *Inventing America*), the very ability of the delegates to the Continental Congresses to mount a common cause was rooted in their Britishness. Different settings put sharply different spins on common ideas and slogans. But the ability of a Thomas Paine, scarcely off the boat from artisan radical London, to tap the core of the Americans' grievances and the long and intimate connection between English and colonial American radicals have brought back to currency Robert Palmer's notion of an age of Atlantic revolution, in which ideas, aspirations, and republican and democratic heresies passed freely between Britain, France, the Americas, and still farther afield.⁴¹

It cannot be said that nineteenth-century United States history has yet been remade in the same way. But some critically important phases of it have already been transformed. Turner's West, for instance. Of the all the contributions of the "new" Western history of the 1980s, the most important has been the insistence on the West, not as a place of escape or rebirth, but as an arena for the projection of metropolitan economic and political power. In Donald Worster's West, the forces of agrarian capitalism call the tune; in William Cronon's, the forces of commodification. In Richard White's recent synthesis, the American West swarms with advance agents of the federal government: military expeditionaries, federal land agents, geological surveyors, and territorial administrators. "Frontier" is not in White's index; one does not meet a homesteader, sodbuster, or farmer until almost a third of the way into the book. The new Western historians' West is fundamentally a place of extraction and exploitation, its mines, forests, animals, and croplands inextricably mixed in a world economy. Its cowboys were ethnically international; its vaunted self-sufficiency was a myth. The American West was not where world systems of economy and politics petered out in virgin emptiness but where, to the contrary, they were etched most clearly.⁴²

As the West has been drawn into global history, so have Crèvecoeur's new immigrant Americans. It had always been a peculiarity of exceptionalist history to imagine immigration as an American-centered story. Even the critical historians had difficulty moving the United States out of the narrative's vital center, reaching deep into a slumbering, tradition-bound, peasant world with promises it would not keep. Within the last decade, however the field has been recast as a phase in the history of global labor migration. In the newest paradigm, the key is not movement to the United States but movement itself in an international labor market of ever enlarging scope. "For America as the special haven of the down-trodden," Virginia Yans-McLaughlin writes, the new immigration histories "substitute America as one point on the periphery of an expanding system of world capitalism."⁴³

The special immigrant character of the United States has all but disappeared. Argentina, Canada, and Brazil were American magnets in their own right; during

the classic period of immigration, 1870–1914, though in raw numbers their immigrant streams never matched those of the United States, both Argentina and Canada had much higher percentages of immigrant to native-born persons in their populations than the United States. The European nations were immigrant nations too, far more porous than conventionally described. Thus between 1876 and 1915, while some eight million Italians left for the Americas, over six million uprooted themselves for other European nations. By the end of the nineteenth century, there were workers on the move everywhere: across Europe, throughout the Atlantic, up and down the Americas, through the Pacific, and virtually everywhere within the great Indian Ocean basin. In this global labor market, a world of diasporas, ghettos, and borderlands, the processes of migration and adaptation did not make the United States different but the same.⁴⁴

More slowly, other parts of nineteenth-century United States history have begun to reflect these broader horizons. Historians of social movements have begun to remap a world of international influences and borrowings, in which the movement of any one country within the Atlantic system could not but leave the others changed. Since publication of the second volume of David Brion Davis's grand work on the history of antislavery, it has been clear that American antislavery cannot be adequately fathomed outside its complicated reciprocal relations with antislavery in Britain and elsewhere.⁴⁵ The history of American religion is an Atlantic history (and by the twentieth century a Pacific history as well); one finds British Methodists in Kentucky, Mormons in London, Zionists in Boston, Presbyterians in China, Buddhists in Chicago.⁴⁶ The nineteenth-century women's movement was an international movement with far-flung international structures and a heavy traffic in aspirations and tactics.⁴⁷ The labor movement, too, can hardly be comprehended except in global terms. Throughout the Atlantic economy, sojourning artisans brought the threads of a recognizable artisan radicalism together; Marxian socialism came to the United States in the heads of German immigrant workers; craft unionism was a successful British importation. The project of establishing the distinctive social origins of American movements of social reform is now being challenged at dozens of sites by a frame of analysis which sees social movements as passed back and forth between interrelated social and cultural systems.

American economic history, long absorbed in the project of understanding the distinctive factor shares in the processes of American economic growth, is ripe for similar reconfiguration: as the story of continuous international flows of capital, technology, skills, labor, raw materials, and manufactured goods into and out of the United States.⁴⁸ The history of foreign policy formation holds out—but with steadily diminishing territory—against international history, where even national boundaries and identities are not always clear.⁴⁹

Of all the aspects of American history, political history clings most strongly to the old exceptionalist story line. Atlantic political-culture studies on the lines of Robert Kelley's *Transatlantic Persuasion* or James Kloppenberg's *Uncertain Victory* remain rare.⁵⁰ Even the political centuries are not the same in and outside American history writing. The European historian's twentieth century is a short one,

from 1914 to 1989; the Americanist's, beginning somewhere in the 1890s, still stretches on past the horizon. But the library of comparative work from which historians of American politics may now draw has expanded dramatically—the best of it blending common and differential threads in designs which confound any simple exceptionalist paradigm. Whether it is the processes of racial segregation in Alabama and South Africa, the conditions of public and private railroad finance in the United States and Prussia, strategies in big business regulation and macroeconomic crisis management, blue- and white-collar political formation, or even social welfare policy development, an American politics isolated from external tendencies and influences becomes ever harder to find.⁵¹ The common forces of politics were not lost to consciousness. American political developments were keenly watched by nineteenth-century Europeans for their portents of the times. In the early years of the twentieth century, American progressives returned the curiosity, scouring Europe for social-political borrowings.⁵²

An overarching conceptual framework for a nonexceptionalist history of the United States is not yet in place. Between the rigidity of Immanuel Wallerstein's world system and the looseness of Michael Geyer's and Charles Bright's, there are conceptual worlds to choose.⁵³ But whatever shape that overview will take, markets and empires (formal and informal) will figure in it far more strongly than they have figured in United States history in the past. Beginning on the rim of an expanding Europe, the trading outpost of distant, commercial empires, the United States grew to preside over a world-spanning commercial empire of its own. Between these beginnings and this destination, the splendid isolation which Americans have wrapped around their history is no longer to be easily found.

The future of such a reading of United States history in a culture still deeply, passionately wedded to exceptionalism is, nonetheless, not easy to predict. The new Western history is bathed in controversy both generational and professional. In the discipline's core journals, as the recent articles by Michael McGerr and Michael Kammen demonstrate, challenges to the exceptionalist paradigm generate sharp, visceral reactions. McGerr worries that the price of the transnational history will be "an estrangement from our audiences." Kammen, in an acerbic rhetoric that belies his moderate conclusion, pits "the newly orthodox homilies" of transnational history against "the solid work . . . [of] judicious and conscientious practitioners." John Higham worries about a history in which the "nation" no longer acts, in which America becomes merely a mere geographic container for processes within and beyond it. "Contrary to much current academic opinion, Ann Douglas opens her recent book, 'I believe . . . that America is a special case in the development of the West.'⁵⁴

Within a political culture which has pinned so many of its ideals to faith in its own uniqueness, there has been a quickness to read challenges to the exceptionalist character of the United States as challenges to those ideals themselves. There ensues a clinging to difference, as if difference were the point in question, a clinging to the terms of exceptionalism even as the conditions which framed exceptionalist historiography pass away. Americans are fond of "the splendid misery of uniqueness," J. G. A. Pocock once wrote with an immigrant scholar's irony;

they might be “happier if they shared their history with other people.”⁵⁵ To give up the imagined rules of everyone else’s history which set off, with artificial brilliance, the uniqueness of American history, to re-embed the history of the United States within a world of transnational historical forces, will not erase the “differentness” of American history—only its imputed immunities and dispensations.

NOTES

1. Byron E. Shafer, ed., *Is America Different? A New Look at American Exceptionalism* (Oxford, 1991).

2. Joyce Appleby, “Recovering America’s Historic Diversity: Beyond Exceptionalism,” *Journal of American History* 79 (1992): 419.

3. J. H. Elliott, *The Old World and the New, 1492–1650* (Cambridge, England, 1970); C. Vann Woodward, *The Old World’s New World* (New York, 1991); Jack P. Greene, *The Intellectual Construction of America: Exceptionalism and Identity from 1492 to 1800* (Chapel Hill, 1993); Karen Ordahl Kupperman, ed., *America in European Consciousness, 1493–1750* (Chapel Hill, 1995).

4. Sacvan Bercovitch, *The Rites of Assent: Transformations in the Symbolic Construction of America* (New York, 1993), p. 30.

5. Appleby, “Recovering America’s Historic Diversity,” pp. 419–20.

6. Robert J. Alexander, *The Right Opposition: The Lovestoneites and the International Communist Opposition of the 1930s* (Westport, Conn., 1981); Bertram D. Wolfe, *A Life in Two Centuries: An Autobiography* (New York, 1981).

7. Michael Lienesch, *New Order of the Ages: Time, the Constitution, and the Making of Modern American Political Thought* (Princeton, 1988).

8. Bercovitch, *Rites of Assent*, p. 8.

9. In an argument which places an “ideology of American exceptionalism” at the very core of the emerging social sciences, Dorothy Ross has emphasized the timeless optimism, the quick and unproblematic extrapolation from present to future tense, the “prehistoricist” understandings of progress and social processes which held in their grip nineteenth- and early twentieth-century history writing. But while the characterizations fit, Ross overdraws their peculiarly American character. Dorothy Ross, *The Origins of American Social Science* (Cambridge, England, 1991). Cf. Daniel T. Rodgers, “Fine for Our Time,” *Intellectual History Newsletter* 13 (1991): 41–44.

10. George Bancroft, *History of the United States, from the Discovery of the Continent* (New York, 1890), vol. 4, p. 450.

11. John Higham with Leonard Krieger and Felix Gilbert, *History* (Princeton, 1965), p. 161.

12. Frederick Jackson Turner, *The Frontier in American History* (New York, 1921), pp. 2, 4, 38.

13. *Ibid.*, p. 281.

14. Edward P. Cheyney, *European Background of American History, 1300–1600* (New York, 1904); Livingston Farrand, *Basis of American History (1500–1900)* (New York, 1904). Turner was assigned *The Rise of the New West, 1819–1829* (New York, 1906).

15. Charles M. Andrews, *The Colonial Background of the American Revolution* (New Haven, 1924).

16. W. E. B. Du Bois, *Black Reconstruction* (New York, 1935); Charles A. and Mary R. Beard, *The Rise of American Civilization*, rev. ed. (New York, 1940), p. 3.

17. Crèvecoeur's own title was more modest: "What Is an American?" J. Hector St. John de Crèvecoeur, *Letters from an American Farmer* (1782; reprint ed., Garden City, N.Y., n.d.). Crèvecoeur's short passage was only one of the canonical texts of the new exceptionalist historiography. Werner Sombart's *Why Is There No Socialism in the United States?* was another—not the text Sombart himself wrote, in the which the special conditions of cheap land and artificially high real wages only temporarily stayed the general forces he saw pressing, in the United States as elsewhere, toward a common working-class politics, but a rather different one, in which Sombart's temporary condition became inherent in the nation's special fortune. Even Tocqueville, whose entire interest in the United States drew on his sense of the interconnectedness of the democratic impulse on both sides of the Atlantic, who saw in Jackson's America not the exception but the rule, the face of France's future, was rewritten, through selective quotation, as a seer of American exceptionalism. Seymour Martin Lipset, "American Exceptionalism Reaffirmed," in Shafer, *Is America Different?* p. 2.

18. Arthur M. Schlesinger, "What Then Is the American, This New Man?" *American Historical Review* 48 (1943): 225–44.

19. Robert E. Brown, *Middle-Class Democracy and the Revolution in Massachusetts, 1691–1780* (Ithaca, 1955).

20. Crèvecoeur, *Letters from an American Farmer*, Letter XII; Thomas Philbrick, *St. John de Crèvecoeur* (New York, 1970).

21. Michael McGiffert, "Selected Writings on American National Character," *American Quarterly* 15 (summer 1963, supplement): 271–88; Michael McGiffert, "The Uses of National Character Studies," *ibid.*, 21 (1969): 330–49; Luther S. Luedtke, "Introduction: The Search for American Character," in Luedtke, ed., *Making America: The Society and Culture of the United States* (Chapel Hill, 1992).

22. David Riesman, *The Lonely Crowd: A Study of the Changing American Character* (New Haven, 1950); David M. Potter, *People of Plenty: Economic Abundance and the American National Character* (Chicago, 1954).

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24. Crane Brinton, *The Anatomy of Revolution* (New York, 1938).

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27. Arthur M. Schlesinger, Jr., *The Age of Jackson* (Boston, 1945).

28. Louis Hartz, *The Liberal Tradition in America: An Interpretation of American Political Thought since the Revolution* (New York, 1955); Louis Hartz et al., *The Founding of New Societies: Studies in the History of the United States, Latin America, South Africa, Canada, and Australia* (New York, 1964).

29. Daniel J. Boorstin, *The Genius of American Politics* (Chicago, 1953), p. 30.

30. Eugen Weber, "Western Civilization," chapter 10 in this volume.

31. William Appleman Williams, *The Tragedy of American Diplomacy* (Cleveland, 1959); William Appleman Williams, *The Roots of the Modern American Empire* (New York, 1969); Lloyd C. Gardner, ed., *Redefining the Past: Essays in Diplomatic History in Honor of William Appleman Williams* (Corvallis, 1986).

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Industrial Revolution in Lynn (Cambridge, 1976); John H. M. Laslett and Seymour Martin Lipset, *Failure of a Dream? Essays in the History of American Socialism* (Garden City, N.Y., 1974); David R. Roediger, *The Wages of Whiteness: Race and the Making of the American Working Class* (London, 1991); Kim Voss, *The Making of American Exceptionalism: The Knights of Labor and Class Formation in the Nineteenth Century* (Ithaca, 1993).

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36. Leon Fink, *In Search of the Working Class: Essays in American Labor History and Political Culture* (Urbana, 1994), esp. p. 28.

37. Joyce Appleby, "A Different Kind of Independence: The Postwar Restructuring of the Historical Study of Early America," *William and Mary Quarterly* 50 (1993): 245–67; Kenneth A. Lockridge, *A New England Town. The First Hundred Years: Dedham, Massachusetts, 1636–1736* (New York, 1970), p. 78.

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Economic History and the Cliometric Revolution

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DURING THE 1960s there was a sharp increase in interest within the American historical profession in borrowing theory and analytical techniques from the social sciences. In part, the motivation for this change was a complementary surge of interest in the experience of “common” people. Because ordinary men and women typically did not leave written accounts of their lives, historians had to learn to extract useful information from dry quantitative sources like vital records and census returns, and they turned to the social sciences for guidance. American historians were by no means unique in this kind of borrowing; indeed, they often consciously followed in the footsteps of their Annalist colleagues in France and of historical demographers in England. More than scholars in Europe, however, historians in the United States turned to the social sciences for a second reason as well—to make the study of the past more systematic and objective, less subject to ideological contamination or bias. As political historian Lee Benson complained, the “profusion of varying interpretations” of American history owed more than anything else to the “near-universal dependence upon impressionistic techniques and data.” He called for historians to formulate “potentially verifiable hypotheses” and engage in the quantitative research needed to test them.¹

Nowhere was this search for rigor pushed further in the 1960s than in the field of economic history. There a small group of economists launched a veritable revolution, seizing control of the discipline’s organizations and using them to build a coherent and uniquely American body of scholarship based on the application of economic theory and econometric techniques to the study of the past.² The new economic historians, or cliometricians as they quickly became known, saw their mission as improving the practice of history by making the articulation of clear, testable hypotheses central to scholarly inquiry. Robert W. Fogel, who received the Nobel Prize in economics for his role in the cliometrics revolution, claimed that traditional economic history was “permeated with untested covert models and subliminal mathematical assumptions.” He was joined by Douglass C. North, the other recipient of the prize, in arguing that economic history should “meet of necessity the same set of standards that we attempt to impose by the use of scientific methods in economics.”³

Although the cliometric revolution initially attracted an enormous amount of attention in the United States and abroad, and in both the disciplines of economics and history, over the long term its impact has been relatively limited. Until

very recently, new economic historians have had few imitators abroad, whereas at home they have become increasingly isolated from the intellectual mainstream in both the economics and historical professions. The aim of this essay is to trace the history of the movement and explore the reasons for its relative lack of influence. Although my narrative will focus as a matter of course on developments specific to economic history, the main outlines of the story can be generalized to other historical fields. The increasing availability of funds for higher education and research, especially in the third quarter of the twentieth century, made it possible for energetic scholars in the United States to build organizations that promoted their particular intellectual visions. Although the resulting associations and journals fulfilled the expectations of their founders by providing support for the new kinds of scholarship they were advancing, these organizations also divided the profession in ways that, in the end, imposed high intellectual costs. The current fragmentation of the American historical profession is the unhappy result.

ECONOMIC HISTORY BEFORE CLIOMETRICS

Economic history as a subject in its own right emerged out of the critique of classical economics posed by members of the (largely German) school of historical economics. Members of this school deplored the deductive methods of modern economics, proposing instead an inductive approach whereby scholars would infer the laws and principles of economic life through careful historical study. Economic history, as opposed to historical economics, was actually a compromise position worked out in Britain during the late nineteenth century, at the same time as marginalists like Alfred Marshall were reformulating classical economics. The compromise in effect recognized the superiority of neoclassical economics for the study of economic choices within a given institutional setting, but left to economic history the study of changes in that setting over time.⁴

The field of economic history had its formal beginning in the United States in 1892 when Harvard created a chair in economic history and appointed British scholar William J. Ashley to fill it.⁵ A number of other universities followed Harvard's lead and established similar chairs during the last decade of the nineteenth century. But though these chairs were situated within the discipline of economics, economic history never gained a solid beachhead in that field. Guy S. Callender complained in 1913 that economists were so absorbed with current events that "topics in economic history found no place upon their programme."⁶ His statement was something of an exaggeration. Most economics departments included at least one economic historian on their faculties, and most required their graduate students to take courses in the field. Nonetheless, economic history clearly occupied a peripheral position in the discipline. During the interwar period, the annual meetings of the American Economic Association rarely included papers on economic history, but instead relegated practitioners to a "roundtable" discussion each year. Nor did the leading journals (the *American Economic Review*,

the *Journal of Political Economy*, and the *Quarterly Journal of Economics*) publish much economic history. Even if one defines the subject matter of the field broadly to include articles on current topics that contain statistical series going back at least twenty years, manuscripts in economic history accounted for only 10 to 15 percent of the articles in the journals. Moreover, the vast majority of the contributions included in this count dealt with very recent topics and contemporary policy issues—not with history proper.⁷

The position of economic history within the historical profession was similarly marginal, but for different reasons. In the same article in which Callender complained about economists' lack of interest in economic history, he commented favorably on developments within the historical profession: "No one who attended the meeting of the American Historical Association in Boston last winter could fail to be impressed by the interest which its members manifested in the economic side of history."⁸ Callender was referring to the newfound preoccupation of Charles Beard and other so-called progressive historians with economic self-interest as a force in historical development. But progressive historians were primarily interested in the connection between economic interests and political events; they were relatively uninterested in studying the economy in its own right. Moreover, there were important ideological differences between the two groups of scholars. Progressive historians typically focused on the negative, exploitative aspects of capitalism, whereas economic historians were more likely to appreciate the material gains that economic development brought and to seek to understand the institutional foundations of that improvement. Consequently, economic history never really found a comfortable home in history departments either.⁹

Despite the lack of a disciplinary home, economic history expanded in the United States during the interwar period and developed its own debates and controversies. Members of the first generation of economic historians—Ashley is a good example—had been trained mainly in constitutional, legal, and political history. Research along these lines continued, but there was also a new effort to expand the use of quantitative economic analysis in historical study. British scholar J. H. Clapham was an important leader of the movement, but in the United States this view was pushed even further by scholars such as Abbott Payson Usher, who argued for a more explicit use of statistics in historical research. Others whose work acquired a quantitative dimension included Wesley Mitchell, who pioneered the study of business cycles, Leland Jenks, who analyzed the migration of British capital to the United States in the nineteenth century, Norman J. Silberling, whose *Dynamics of Business: An Analysis of Trends, Cycles, and Time Relationships in American Activity Since 1700* was, after many years of effort, finally published in 1943, and Arthur D. Gayer, Walt W. Rostow, and Anna Jacobson Schwartz, who launched a study of the British economy comparable to Silberling's.¹⁰ New and existing organizations provided critical financial support. The Rockefeller Foundation granted \$250,000 to form an International Committee to study changes in price levels over time. Edwin F. Gay and Wesley Mitchell jointly organized and headed the National Bureau of Economic

Research, whose founding principle was the belief that research in economic history, particularly the careful collecting of long-term quantitative data sets, provided a vital foundation for policy making. The newly created Commission on Recent Economic Changes, the Commission on Recent Social Trends, and the Social Science Research Council had similar motivations.¹¹

Despite this activity, the American branch of the field was still largely dependent on European scholarship for ideas and support. Many of the nation's early practitioners had been trained abroad. William J. Ashley, the first occupant of Harvard's chair in economic history, was, of course, English. Edwin F. Gay, Ashley's successor and a leader of the field in the interwar period, had studied in Germany. These men, and others like them, introduced American scholars to German stage theories of economic development and to the English idea that the modern era of economic growth had originated in a "take-off" which Arnold Toynbee called the "industrial revolution." The interwar generation of quantifiers were similarly inspired by the work of Clapham and also by the founders of the Annales school on the Continent.¹² Indeed, the prestige of European scholarship was so pronounced that, as late as 1940, one major scholar (quoting another prominent economic historian) was able to declare that "American economic history . . . has been less interesting to read than European economic history."¹³

One could argue, in fact, that the dominance of European scholarship delayed the formation of a disciplinary apparatus in the United States. Many American scholars were members of the Economic History Society, which was organized in Britain in 1926, and they contributed actively to that society's *Economic History Review*. Although some scholars, especially those working on American topics, began to argue in the 1930s that the United States should have its own professional organization and journal, others thought that the resulting fragmentation would weaken the discipline, and this position easily carried the day during the Great Depression, when resources for new academic initiatives were scarce.¹⁴

Ultimately, however, the outbreak of war in Europe provided the impetus for change. As Herbert Heaton later explained, "If research, monographs, and periodicals were doomed to be blacked out in Europe, the lights must burn more brightly in America; and if old lamps were not to be re-lit, new ones must be made."¹⁵ The Economic History Association was the product of two separate initiatives in 1939—one by historians who took the opportunity afforded by the American Historical Association (AHA) meeting of that year to organize an Industrial History Society, the other by economists associated with the American Economic Association (AEA). The latter group, whose executive committee consisted of Arthur H. Cole, Herbert Heaton, Earl J. Hamilton, and Anne Bezanson, proved the more energetic. They polled the membership of the two larger associations (getting more than 400 positive responses to 500 mailings) and arranged for joint meetings of the proposed Economic History Association with both the AHA and AEA in 1940, at which gatherings the new organization was ratified. The Economic History Association claimed 361 members at its first meeting.¹⁶

The formation of an American association was accompanied by a burst of related activity involving many of the same leading scholars. One of the most im-

portant results was the creation of the Committee for Research in Economic History (CREH) in the winter of 1940–41 with the support of the Rockefeller Foundation.¹⁷ The committee determined to play an active role in the profession by channeling research funds to projects it commissioned. Its first major effort was to fund a series of studies on the role of government in American economic development, a project that generated Oscar Handlin and Mary Flug Handlin's *Commonwealth: A Study of the Role of Government in the American Economy: Massachusetts, 1774–1861* (New York University Press, 1947) and Louis Hartz's *Economic Policy and Democratic Thought: Pennsylvania, 1776–1860* (Harvard University Press, 1948). The committee also became the locus of planning for a new Research Center in Entrepreneurial History, which was founded, again with Rockefeller seed money, at Harvard in 1948. The center became a magnet for scholars interested in understanding the sources of entrepreneurship and why certain societies are more innovative than others.¹⁸

Although the scholars who organized both the Economic History Association and the Committee for Research in Economic History were mainly economists, the studies sponsored by the latter group in particular brought in a wide range of other participants and moved the discipline away from its original location in economics departments. The project on the role of government in the economy attracted historians like the Handlins and political scientists like Hartz, who had the training and inclination to delve minutely into the policy debates of the early nineteenth century. The effect of the Research Center in Entrepreneurial History was even more profound. Although many of the scholars at the center were economists, others felt that neoclassical theory had little to contribute to the study of entrepreneurship. After an active search for a usable alternative, they turned to Parsonian sociology instead. The work of some of the most important scholars associated with the center—good examples are David Landes, Thomas Cochran, and Alfred D. Chandler, Jr.—consistently employed concepts and addressed debates at the heart of this sociological literature, even when they did not make extensive use of its rather arcane vocabulary and categories of analysis.¹⁹ As a result, then, of the committee's activities, by the mid 1950s—that is, by the eve of the cliometric revolution—many of the field's most active practitioners no longer retained close intellectual ties to the discipline of economics.

THE CLIOMETRIC REVOLUTION

The cliometric revolution would have been impossible without this period of organization building, for otherwise there would have been nothing for the “Young Turks” who launched the movement to take over. It is also difficult to imagine the revolution occurring without the increased flow of research funds, from both governmental and private sources, that occurred in the post-Sputnik period. Moneys were suddenly available to support initiatives in the hard social sciences, and the early cliometricians made effective use of them. Lance Davis, Jonathan R. T. Hughes, and Duncan McDougall, all young faculty members at

Purdue, took the lead and secured funding in 1960 for a series of cliometrics conferences held annually at that institution for the next nine years. After the three original organizers left Purdue, the conference moved to the University of Wisconsin, then to the University of Chicago, and then to other locations. In 1985 participants in a "World Congress" held at Northwestern University ratified the creation of a permanent association, the Cliometric Society, to promote econometric history. The society continues to hold annual meetings funded by the National Science Foundation.²⁰

The first few Purdue meetings were extraordinarily important in forging cliometricians into a cohesive group. The meetings quickly became known for both their feisty criticism and their camaraderie. As Jonathan Hughes later recollected, at Purdue "all the best parties took place when the economic historians were in town. . . . [I]t was New Year's Eve for several days."²¹ Fogel, who was just beginning his first teaching appointment (and still writing his dissertation) when he attended the first (1960) meeting, recalled the "tremendous excitement and exhilaration on the part of everybody" who was present.²² According to Robert Gallman, it was these events that created "a special cliometrics group with a sense of identity." The meetings transformed his career: "Before I went to the first Purdue meeting, I thought of myself as a development economist of a Kuznetsian variety." But his self-definition changed after attending a couple of the "clio" sessions: "Discovering that there was a group of scholars who were interested in the full range of issues that had captured my imagination and who were at work on really creative, useful research along these lines was the most exciting discovery of my scholarly career."²³ In actuality, the participants already had much in common. Most had been heavily influenced by the growth economics literature associated with Simon Kuznets, and many had been Kuznets's students.²⁴ But the Purdue meetings created a heightened "sense of intellectual communion," to use William Parker's phrase, that carried over into other projects—for example, the multiauthor textbook *American Economic Growth: An Economist's History of the United States*, published by Harper & Row in 1972. By the time of that collaboration, Parker felt "we really were quite a little group."²⁵

Like all revolutionaries, the cliometricians advanced a narrative of their origins that denigrated the achievements of their predecessors and exaggerated the intellectual distance they had come. In a 1963 communication published in the *American Economic Review* Douglass C. North proclaimed that "a revolution is taking place in economic history in the United States" and went on to justify in sweeping terms the overthrow of the old regime. "Even a cursory examination of accepted 'truths' of U.S. economic history suggests," he asserted, "that many of them are inconsistent with elementary economic analysis and have never been subjected to—and would not survive—testing with statistical data."²⁶ Two years later he elaborated the point, summarizing the "deficiencies of economic history" as previously practiced:

- (1) Vast areas of economic history have not been treated at all; that is, treated in the sense that economic theory and statistics have been used to examine the past.

(2) Many writings in economic history are loaded with statements which have economic implications and imply causal relationships which are not only not supported in the research but which run counter to basic economic propositions. In fact, in most such cases, the author appears to be completely unaware of these implications. (3) Even more conspicuous is the character of the evidence advanced to support propositions. In good part it consists of a mishmash of quotations and oddly assorted statistics which do not provide any support or test for the propositions developed. (4) A good deal of economic history draws broad welfare conclusions which are by no stretch of the imagination warranted from the evidence cited. In fact, a general characteristic of economic history is that the treatment of propositions with broad welfare implications is typically undertaken without even a token acquaintance with welfare economics.²⁷

Both North and Fogel showed their contempt for earlier work by playing “games” in their classes. North would ask his students to develop explicit models to capture the arguments made by traditional economic historians, and claimed that “even by plugging into each model the most favorable possible implicit assumptions, most of the resultant models turn out either to be internally inconsistent or to run counter to the most fundamental propositions in economics.”²⁸ Fogel would challenge his students “to pick any page at random from whatever history book they had at hand. The odds were . . . that there’d be either an explicit or implicit quantitative statement that needed to be measured.” Fogel later claimed that “the challenge was often taken up and I was never shown up.”²⁹

Most of the early leaders have since moderated their views, and indeed, more than a third of a century after the fact, the elements of continuity between the “old” economic history and the “new” appear much more important than they undoubtedly did to contemporaries. For example, one of the hallmarks of cliometrics was the rigorous quantitative testing of hypotheses, but, as we have seen, the interwar generation of economic historians had already moved a long way in this direction. Paraphrasing Clapham, Herbert Heaton complained at the time of the founding of the Economic History Association that the field “in its early stages . . . [had] suffered from an overdose of generalizations based on scanty data.” By contrast, he claimed, the “urge . . . to answer such questions as How much? How many? How quickly? or How representative? is perhaps the outstanding characteristic of our generation.”³⁰ Similarly, E. A. J. Johnson, first editor of the *Journal of Economic History*, used the bully pulpit associated with his position to call for more explicit use of economic theory in historical work and urged economic historians to use theory as a defense against the “fascination of antiquarian details.”³¹

John Meyer, recalling the reaction to his and Alfred Conrad’s controversial “The Economics of Slavery in the Antebellum South,” presented in 1957 at a joint meeting of the Economic History Association and the Conference on Research in Income and Wealth, cautioned that “it’s easy to overestimate the hostility of it. The hostility was fairly limited. Most [of the old economic historians] were really quite open-minded and responsive.”³² As Robert Gallman recalled that meeting and another joint undertaking the next year, there was no “general division

between cliometricians and traditionalists,” and several of the latter gave “thoughtful and friendly reviews” of cliometric papers.³³ Certainly, some of the leading members of the profession actively encouraged the young cliometricians. Arthur Cole offered the position of director of the Research Center in Entrepreneurial History to Douglass North in 1954, and tapped John Meyer for the role of acting editor of *Explorations in Entrepreneurial History* (the journal started by the Research Center) in 1957. In 1959 Frederick C. Lane put North on the council that replaced the Committee on Research in Economic History, and the traditional economic historians who constituted the board of trustees of the Economic History Association chose North and William Parker to be coeditors of the *Journal of Economic History* in 1960.³⁴

There is no doubt, however, that others were much less tolerant of the new work. Lance Davis later recalled, “It certainly was difficult to get quantitative work published; and I had my share of losing bouts with George Rogers Taylor,” then editor of the *Journal of Economic History*.³⁵ Fogel has similarly claimed that journals “initially refused to accept articles with complex tables, and even after such articles began to be accepted, equations were forbidden.”³⁶ At a conference at the Hagley Museum and Library, Fritz Redlich ranted to Gallman about “that madman Fogel,” who “plans to build canals across the Appalachian Mountains.”³⁷ Redlich was not alone in his view of Fogel’s work, and after North and Parker began publishing cliometric work in the *Journal of Economic History*—particularly a Fogel article on railroads—several of the trustees of the Economic History Association moved to get them fired. Parker appeared before the trustees to explain the editors’ decisions, and the outcome, as North recollected later, was “we got impeached but we didn’t get fired; finally, they went back and agreed to continue us, even though with some reluctance on quite a number of the trustees’ parts.”³⁸

Claudia Goldin has argued that the opposition of traditional economic historians to the new economic history arose in large measure because the conclusions of early cliometric work ran counter to orthodoxy. As she put it, “there was already a huge fossilized stock of accepted wisdom concerning major projects, figures, and events of the past,” and as a result, cliometrics inevitably involved “a major challenge to an entire field.”³⁹ Such a view, however, overstates the coherence of the discipline of economic history before cliometrics—divisions between quantifiers and institutionalists went back at least as far as Clapham and Usher’s interwar salvos.⁴⁰ In addition, much of the conventional wisdom that cliometricians attacked was not in fact the work of economic historians but instead of progressives and other mainstream historians. One of the primary targets of Peter Temin’s *The Jacksonian Economy*, for example, was Arthur Schlesinger, Jr.’s *Age of Jackson*.⁴¹ Similarly, it was the progressive view of the Populists that North critiqued in *Growth and Welfare in the America Past*.⁴² Moreover, even when cliometricians clearly targeted the work of older economic historians, there were often other traditionalists who held views comparable to their own. For example, though the “axiom” of indispensability that Fogel attacked in *Railroads and American Economic Growth* could be attributed to W. W. Rostow and Leland Jenks,

contrary views (to which Fogel himself was intellectually indebted) had been promoted by earlier scholars such as Kent T. Healy and Carter Goodrich.⁴³

Historians like Redlich were less enraged by Fogel's attack on the conventional wisdom than they were by his method—in particular, his notion of the counterfactual.⁴⁴ Fogel argued that anyone who asserted the indispensability of the railroad to American economic development was implicitly rejecting the hypothesis that the American economy could have attained essentially the same level of economic well-being in the absence of this particular transportation innovation. His aim was to test the counterfactual hypothesis explicitly by calculating the social savings of the railroad over alternative means of transportation that already existed or alternatively could have been built. To Redlich, this was a wrong-headed exercise. Fogel was investigating “what would have happened in the event that something else had happened which . . . *could* not have happened.” In his view, there was a logic, a direction, to technological change: “Once the atmospheric engine had been developed into an efficient steam engine and the steam engine had successfully been put into boats, . . . it was only a question of when the steam engine would be put on wheels, particularly as the railroad minus locomotive had existed for a long time.” Fogel was not contesting the idea that cheap transportation was necessary for the economic development of the United States; so it was a rather silly exercise, Redlich thought, to question the importance of the particular form of cheap transportation that technological logic had produced.⁴⁵

Other historians accepted the validity of Fogel's notion of the counterfactual hypothesis, but looked askance at the comparative static methodology he used to test it. What Fogel did, in effect, was compare the costs of shipping various goods by railroad and water in 1890, after making some clever adjustments for the greater speed and safety of shipment by rail. As Fogel himself admitted, the calculation ignored possible dynamic consequences of the innovation: “the model [was] not designed to deal with other important issues such as the effect of transportation improvements on the spatial location of economic activity, induced changes in the industrial mix of products . . . , induced changes in the aggregate savings rate, and possible effects on either the rate of technological change in various industries or on the overall supplies of inputs.”⁴⁶ Yet it was changes like these that the historians who rallied to reassert the railroads' role in American economic development thought were important. Reviewing Fogel's book, for example, Louis Hacker questioned whether water transportation rates would have declined as dramatically as they actually did in the absence of the railroad overbuilding: “If . . . the bitter struggle for markets of the railroad promoters had not occurred through building and overbuilding, would water rates have gone down so sharply?” He then moved on to bigger issues. “Only railroads,” he suggested, “could carry swiftly public supplies and personnel (soldiers) into and across the vast unsettled country. . . . [H]ow could the continental United States have become a single, unified nation?”⁴⁷

Perhaps more important, the debate over Fogel's work highlighted real and important differences in approach between the old and new economic historians

that were obscured by all the rhetoric about formal testing of hypotheses. Although Redlich himself was not theoretically oriented, most of the historians at the Harvard Research Center were. Their starting point was Joseph Schumpeter's concept of entrepreneurship as a creative act that in discontinuous fashion altered—shifted outward—the economy's production possibility frontier.⁴⁸ Entrepreneurship was important to study, they argued, because these discontinuous creative acts were the key to greater social well-being. As already discussed, many of them found conventional neoclassical price theory to be of limited utility in this endeavor, and they turned instead to role theory and other sociological models of human behavior in order to understand what motivated entrepreneurs. By contrast, Fogel and other new economic historians subscribed to the neoclassical view that technological innovation was induced by changes in relative prices—that is, by market-driven opportunities for profit. Thus Fogel not only postulated that the canal system would have (and could have) expanded to meet the demand for transportation services; he also raised the possibility that automobiles would have been developed earlier in the absence of railroads. Inventors had been experimenting with steam-powered carriages in the 1820s, and the theory of both the internal combustion and diesel engines had been published by 1824. As Fogel pointed out, "The axiom of indispensability proceeds on the implicit and unverified assumption that the success of railroads did not choke off the search for other solutions to the problem of overland transportation."⁴⁹

Other leading cliometricians shared Fogel's view that technological innovation was largely a response to demand-side stimuli. As William Parker later explained, a good part of "what the new economic history is about . . . is a gigantic test of the hypothesis of economic rationality. . . . There is not much room here for good and bad entrepreneurs, leaders and followers."⁵⁰ Despite his position as editor of *Explorations in Entrepreneurial History*, John Meyer was, to use his own recent characterization, "very skeptical of the importance of any intangible, such as entrepreneurship" and committed "some of that skepticism to paper."⁵¹ Similarly, Douglass North explicitly downgraded the role of the entrepreneur in his *Economic Growth of the United States*, arguing instead "that productivity changes stemming from technological innovations are, in part at least, a nearly automatic response to successful expansion of industries in an acquisitive society under competitive market conditions." Although North qualified his generalization by emphasizing its application to "economies which: (1) followed in the process of industrial development, and (2) were acquisitively oriented under competitive market conditions," the central point was clear. He saw no reason to devote time or resources to studying the entrepreneurial function in American business. For example, though the cotton gin was in North's view "unquestionably the most significant invention during the years between 1790 and 1860," one could learn little by studying Eli Whitney. The cotton gin was the product of a "concerted search" for a solution to the South's economic dilemma "that the demand for its traditional staples was no longer increasing and its heavy capital investment was in slaves." If Whitney had not invented it, someone else would have.⁵²

What the new economic historians did, in a nutshell, was to upset the long-standing division of labor between economics and economic history—a division of labor that relegated marginalist (neoclassical) economics to the study of short-term phenomena.⁵³ Not all traditional economic historians had been atheoretical; on the contrary, many of them had devoted their lives to studying theory and developing systematic ways of understanding the long-term changes with which they concerned themselves. They had simply turned to other bodies of theory besides neoclassical economics for their models. By contrast, cliometricians consciously aimed to expand the domain of neoclassical economics by emphasizing the pervasiveness of the market processes that this brand of theory was so well suited to analyze. They even formulated a new theory of institutional change in which rational economic actors would organize to secure change if the benefits promised to outweigh the costs of the organizational effort.⁵⁴

In subsequent years, more and more cliometricians would become dissatisfied with this expansive approach and again proclaim the limitations of neoclassical economics for the study of institutional change. North dubbed the earliest of these critics the “Harvard Wing” because many members of the group had studied with Alexander Gerschenkron at that institution. By the early 1970s, however, North himself had joined their ranks. As he stated in his 1974 address as president of the Economic History Association, “Neo-classical economic theory has two major shortcomings for the economic historian. One, it was not designed to explain long-run economic change; and two, even within the context of the question it was designed to answer, it provides quite limited answers since it is immediately relevant to a world of perfect markets.”⁵⁵ Reviewing two decades later the achievements of cliometrics, North admitted, “What we did then was impressive enough to be called a revolution, but the failure to go on to deal with the two major shortcomings of neoclassical theory applied to history have aborted the revolution.”⁵⁶ From the early seventies on, North for the most part abandoned cliometric work and devoted his energies to developing a general theory of institutional change that would transcend the limits of neoclassical economics.⁵⁷

BOUNDARIES OF CLIOMETRIC INFLUENCE

The iconoclasm of the cliometricians attracted new practitioners to the field and inaugurated a period of rapid growth for the Economic History Association. As late as 1959, the association's individual members numbered 476, only 32 percent more than in 1941. By 1965, however, the number had grown to more than 800.⁵⁸ Most of the new members were economists, and the association took on an increasingly cliometric tone. As Robert Whaples's quantitative analysis of the *Journal of Economic History* has shown, the cliometric revolution was accompanied by a dramatic shift in the subject matter of the journal away from business history, the history of economic thought, and banking, in favor of studies of

economic growth, trade, and industrialization. The proportion of the journal devoted to cliometric-type articles also increased—from 10.2 percent in 1956–60 to 42.8 percent in 1966–70 to 71.8 percent in 1971–75.⁵⁹ At the same time, the cliometricians gained control of *Explorations in Entrepreneurial History*, which they renamed *Explorations in Economic History* in 1969, and which now defined its target audience to be scholars trained in economics.

While all this was going on, economic historians trained in history increasingly retreated to a new organization called the Business History Conference. The conference had its origin in a series of meetings (the first was at Northwestern in 1954) that brought together economic and business historians who were rebelling against the atheoretical type of scholarship promoted by N. S. B. Gras at the Harvard Business School. The group met twice in 1954, once in 1956, once in 1958, and then yearly thereafter, and in 1971 it transformed itself into a full-fledged professional association with dues, officers, a board of trustees, and a journal (albeit one that published only a single issue a year). Although many of its original members were economists, during the 1970s the conference increasingly provided historians fleeing the cliometric revolution with an intellectual home. To the present day, the Business History Conference is dominated by scholars trained in history, whereas the Economic History Association is controlled by economists; only a small number of scholars attend both meetings.⁶⁰

Outside the United States there was no comparable transformation in the practice of economic history. The overwhelming majority of participants in the Purdue conferences had appointments in American academic institutions, so the movement never produced a sizable cadre of foreign scholars who could go back to their home countries and influence the direction of research there. Instead the spread of cliometrics abroad depended in large measure on its intellectual appeal, which in turn was limited by the perceived narrowness and conservatism of the neoclassical models on which it was based. Latin American economists, for example, were subjecting neoclassical theory to extended critical attack at the very moment that cliometrics was gaining ground in the United States. The result was the rise of dependency theory, whose fundamental tenet that capitalist exchange was the source of underdevelopment in the Third World—that is, that markets do *not* work for the greater good of all—was diametrically opposed to the assumptions of cliometrics.⁶¹ Similarly, as George Grantham has pointed out, cliometricians' emphasis on economic rationality may have limited its appeal in France, where academics typically had more training in philosophy than was common in the United States and where more philosophically sophisticated disciplines like structural anthropology consequently had greater influence. But it is also important to recognize that economic history never had had much of a presence in France—as late as 1960 there was only one chair in the field—and the dominance of the more eclectic *Annales* school may have stymied its development thereafter.⁶² In Germany, the antitheoretical bias that derived from historical economics was a continuing factor inhibiting the spread of cliometric work there. As Richard Tilly put it, "German economic historians, increasingly producing economic history without economics, have been playing Hamlet without the

Prince.” He also pointed out that economic historians in Germany had largely transformed themselves into social historians, and that little economic history was actually being written at the time of the cliometric revolution in the United States.⁶³ In Britain, the situation was somewhat different. The Clapham tradition of empirical research continued strong, but by the time of the cliometric revolution most practitioners were trained as historians rather than as economists and were predisposed to regard cliometric work as narrow. For example, although Peter Mathias recognized the validity of Fogel’s call for rigorous testing of hypotheses, including implicit counterfactual ones, he argued that the inevitable narrowing of focus that such a pursuit of rigor required would “paradoxically” strengthen the position of historians like himself who aim “to see things in the round.”⁶⁴

This is not to say that cliometrics had no early followers abroad. Roderick Floud proselytized for the new economic history in Britain with modest success, and Maurice Lévy-Leboyer’s experiments with econometric techniques stimulated a minor wave of interest in France.⁶⁵ Although the immediate impact in both cases was limited, in recent years as American-style neoclassical economics has increasingly spread internationally, so has cliometrics. The second “World Congress” of cliometrics held in Santander, Spain, in 1989 still attracted nearly two-thirds of its attendees from the United States and Canada, but there were thirteen participants from Spain and sixteen others from Western Europe. The growth of interest since then, moreover, has been impressive, and the Cliometric Society now claims forty-one European members, up from six in 1990.⁶⁶ The expansion has been particularly marked in Great Britain, where approximately 50 percent of the articles published by the *Economic History Review* in 1994 could be considered cliometric.⁶⁷ French academia has been enlivened by rising new economic historians such as Jean-Michel Chevet, Gilles Postel-Vinay, and Pierre Sicsic, and the University of Munich attracted John Komlos from the United States in order to develop a cliometric presence in Germany. In the mid-1990s, moreover, European cliometricians took steps to organize their own association and scholarly journal.⁶⁸

Ironically, within the United States, the trend in the influence of cliometrics has been just the opposite, with both economists and historians losing interest in the new economic history. Economists had originally found the cliometric revolution intriguing. The breakdown of the traditional division of labor between economics and economic history awoke their interest in historical topics. In addition, economics was itself undergoing an econometric revolution at the same time, and cliometricians’ demonstration of the utility of new techniques for historical research suited the imperialism of the econometricians. By the mid-1970s, however, economics as a discipline had become more theoretical and mathematical in its orientation, and applied work in general suffered a decline in influence and in resources. Because they had to devote a great deal of their training to acquiring historical knowledge and research skills, few economic historians were able to remain at the cutting edge of quantitative work. As a result their studies held less and less interest for econometricians. Compared with other applied

fields like labor economics, moreover, research in economic history appeared to have fewer direct policy implications. As a result, economic history increasingly came to be seen as an unaffordable luxury, and departments began to cut positions and eliminate required courses from the curriculum.⁶⁹

There has been a similar loss of interest on the historical side, although for very different reasons. Initially, many mainstream historians—like many of the traditional economic historians in the Economic History Association—regarded the work of cliometricians with guarded interest. Worried about a lack of rigor in historical research, they applauded the attempt to import research methods and theories from the other social sciences. From their perspective, the new economic history was not fundamentally different in thrust from the new social history or the new political history that were flourishing at the same time. Moreover, the assumptions about economic rationality that most cliometricians shared were not seriously at variance with the assumptions about the popular acceptance of capitalism that underpinned consensus history, the dominant intellectual tradition at the time. Because, however, the theories and quantitative techniques employed by economists were more difficult to master than those of the other social sciences, few historians attempted this kind of work themselves. In effect, they abandoned the field to scholars whose training was primarily in economics.⁷⁰

The regard in which many historians initially held cliometric work was undermined, however, by developments within the historical profession during the 1960s, particularly the growth of “bottom up” and “New Left” history. Those who wrote about the “underside” of history started from the premise that economic development had dire consequences for the bulk of the laboring population. They had little sympathy for the idea that the market works for the general good. Similarly, those who wrote history from the New Left perspective focused on exposing ways in which business interests (with the help of government) had manipulated the market for their own ends. Perhaps more important, the attack that historians of both these schools launched against consensus history inevitably undermined faith in historical objectivity, and with it the belief that social science methods could make the research process more rigorous. Neither careful research nor borrowings from social science theory, these scholars claimed, had prevented consensus historians from infusing their work with their own political preferences and class biases. Moreover, consensus historians could respond in kind, accusing their challengers of “present-mindedness” and pursuing a political agenda in their scholarship.⁷¹

The idea of historical objectivity suffered further assaults with the rise of black and women’s history, and it was just when all these new movements were peaking that Robert Fogel and Stanley Engerman came out with *Time on the Cross*.⁷² The authors made broad claims for the book’s scientific character. Following the formula that Fogel had used so successfully in his railroad study, the book began with a dramatic list of “principal corrections” to the “traditional characterization of the slave economy” that had emerged as a result of the application of “mathematics and statistics in historical analysis.”⁷³ However, the authors’ strategy for presenting their findings seemed to belie their claim to the mantle of science.

Time on the Cross was published in two volumes. The first consisted of a summary of the authors' findings, written for maximum popular impact and lacking the scholarly edifice of notes and documentation that other historians would need to begin the scientific process of verification. The authors claimed that such documentation was contained in the second volume, but historians were dismayed to find not only that "the arrangement demand[ed] inordinate time and patience of the reader" but that, in fact, it was "extraordinarily difficult to associate the argument with the proof on which it rest[ed]." ⁷⁴ Moreover, to many historians the ideological biases of the book appeared to be, if anything, more pronounced in *Time on the Cross* than in other cliometric work. Once again, Fogel and Engerman asserted, systematic hypothesis testing confirmed that the market worked: slave-based agriculture was a profitable, efficient system; economic incentives encouraged planters to provide adequate food, clothing, shelter, and medical care to their slaves, and, as a result, the proportion of the slaves' product expropriated by their masters "was much lower than has generally been presumed"; "it was to the economic interest of planters to encourage the stability of slave families and most of them did so," limiting slave sales largely to whole families or to ages when it would have been "normal" for children to leave home; finally, planters recognized the value of (and used) positive incentives to induce their slaves to labor more intensely, and slaves responded to these carrots with greater effort. ⁷⁵

Although most historians found at least some of these claims objectionable, for the most part they were acquiescent in their reviews of the book. How, after all, could they help but be intimidated by Fogel and Engerman's references to hoards of research assistants "searching out and systematically sifting through huge quantities of data" using new "advances in economics, statistics, and applied mathematics, together with the availability of high-speed computers." ⁷⁶ Fogel and Engerman's fellow cliometricians were not so reticent, however. In a series of meetings and conferences, new economic historians like Paul David, Richard Sutch, Peter Temin, and Gavin Wright mounted a critique that challenged the book on every level—from its philosophical underpinnings to the correctness of its economic theory to the appropriateness of its quantitative tests to its handling of historical data. ⁷⁷

The cliometricians offered their critique "as a scientific contribution to the writing of American history"—that is, as an exercise in "replication" that fulfilled the scientific injunction to subject the work of colleagues to the "recognized methodological standards of the discipline"—but many historians drew a very different lesson from the exercise. ⁷⁸ After watching cliometricians rip the book apart, they concluded that the new economic history was no more or less scientific or objective than any other kind of history. Kenneth Stampp underscored the lessons to be drawn from the debate: "History is not an exact science—not even the 'New Economic History' with its immensely valuable methods of quantification and data analysis. Ultimately the most meticulously weighed and finely measured data, both numerical and literary must be subjectively interpreted by the historian, for historical facts do not speak for themselves." ⁷⁹

Some of the critics of *Time on the Cross* feared that the problems they had uncovered in Fogel and Engerman's work would provide "a perfect foil for those skeptical of efforts to employ techniques and methods of the social sciences in the reconstruction of the past."⁸⁰ Their worry turned out largely to be correct. Historians generally missed the fact that it was precisely Fogel and Engerman's careful framing of hypotheses and use of quantitative data—and their generous willingness to make their research available to critics—that made it possible for other economic historians to test their views. They also missed the point that even a cursory comparison of the literature before and after the *Time on the Cross* debate shows how much our understanding was advanced by the whole process.⁸¹ Instead, when news of the cliometricians' attack on *Time on the Cross* spread through the profession, historians tended to throw up their hands and turn their backs on the discussion. Historical work on slavery was already moving in the direction of cultural studies, and from this point on historians displayed very little interest in the economic dimensions of the institution of slavery.

This reaction itself cries out for explanation, however. After all, debates over interpretation and even substance are staples of historical scholarship, and typically fuel more discussion rather than generate silence. Why in this case were the consequences so dire for interdisciplinary dialogue? Thomas L. Haskell's assessment of the debate in the pages of the *New York Review of Books* offers important clues. What bothered Haskell in particular was the contrast between the surface impression that cliometrics was "an austere and rigorous discipline that minimizes the significance of any statement that cannot be reduced to a clear empirical test" and what he called its "soft, licentious side." This contrast, he believed, owed "paradoxically" to cliometricians' reliance on mathematical equations. Although this reliance enabled economic historians to give precise expression to their hypotheses, it also forced them, in the face of a necessarily incomplete historical record, to estimate missing data by making a variety of assumptions that ultimately depended on the same kind of intuitive feel for the data as traditional historical research. Moreover, although cliometric methods required that these assumptions be made explicit, they set no limits on the number of assumptions that could be made or how high they might be piled.⁸²

So long as historians had faith that cliometricians' methods were more objective than those of other historians, they had willingly allowed the new economic historians to specialize in topics for which their training gave them a comparative advantage and then incorporated the results into their own view of history. The *Time on the Cross* debate, however, convinced many historians not only that this faith had been misplaced but that the mathematical expressions in which many cliometric findings were couched were particularly misleading. Because they did not themselves have the knowledge or skills to determine the effect on the models of changing assumptions they found unreasonable, they tended to dismiss the whole literature as suspect, a reaction that was reinforced by the underlying ideological divisions between cliometricians, on the one hand, and most historians, on the other. Now, moreover, the negative side of the cliometricians' institution building became strikingly apparent. Because economic historians had organized

themselves so effectively into a separate subdiscipline, their mainstream colleagues in history had to make a conscious effort to follow developments in the field. After the mid-1970s historians, convinced that cliometrics had little to offer them and increasingly preoccupied with building up their own subdisciplinary organizations, stopped making the effort—stopped reading the *Journal of Economic History* and stopped following developments in the field.

The gulf that resulted between economic history and history proper has clearly been detrimental to scholarship. Not only have the two groups of academics deprived themselves of the benefits of cross-fertilization of ideas, but because practitioners on either side of the divide have failed to keep abreast of developments on the other, they have not upheld the profession's minimal standards of scholarly competence. Because keeping up with the economic history literature required more effort for historians than the reverse did for cliometricians, the negative consequences have been particularly apparent on the historical side. Thus many historians of the United States continue to teach the view of the Great Depression that John Kenneth Galbraith popularized in his 1955 book *The Great Crash*, complacently ignoring the voluminous literature on Federal Reserve monetary policy and the banking crises of the early 1930s that has accumulated since then and seemingly unaware that a powerful new interpretation connects the depth and severity of the depression to adherence to the gold standard.⁸³ Similarly, most historians seem to be completely unaware that the optimistic consensus about standards of living during the industrial revolution has been seriously challenged by new work on nutrition and mortality stimulated by Fogel.⁸⁴ Historians, moreover, continue to evince an often painful naiveté about economic concepts, equating, for example, market behavior with the narrow pursuit of profits and suggesting that the mere existence of markets can somehow force a supply response from unwilling participants.⁸⁵

THE "LINGUISTIC TURN" IN HISTORY AND ECONOMICS

In recent years this gulf has grown even wider as a result of a shift in intellectual fashions in favor of cultural, as opposed to social and economic, history. This shift has effectively redefined historical studies "as the investigation of the contextually situated production and transmission of meaning" and inspired historians to take a "linguistic turn"—that is, to turn to literary theory rather than the social sciences for inspiration and guidance.⁸⁶

Ironically, the discipline of economics has also recently been transformed by a series of theoretical developments that parallels in intriguing ways the emergence of critical theory in the humanities. Abandoning the convenient but unrealistic assumptions of traditional neoclassical theory—in particular the assumption that all economic actors make decisions on the basis of perfect information—economists have begun to reconceptualize the world as a place where information is scarce, imperfect, and costly, where people build institutions in order to cope with problems of imperfect information, where human beings'

“bounded rationality” affects their economic decision making, and where economic processes can have multiple outcomes depending on participants’ perceptions of each other’s actions.⁸⁷

The questions at the heart of this new work—how do economic actors know what (they think) they know, and how does what (they think) they know affect their behavior?—are remarkably similar to those that inform the work of the new cultural historians. But, of course, the theorists who are participating in this intellectual movement are interested, as is their wont, in developing general economic models that capture the new assumptions about information and in exploring the implications of these models under a variety of circumstances. The models they build are highly abstract and mathematical and, to the uninitiated observer, appear to bear little or no connection to actual circumstances, whether current or historical. The purposes of these scholars are thus very different from those of historians, who are more interested in understanding specific historical phenomena. Indeed, the intellectual agendas of the two disciplines appear to be so dissimilar that it might seem doubtful whether, on their own, practitioners could ever come to appreciate, let alone learn anything, from each other’s work.

Economic historians are well positioned, however, to bridge the gap, and in recent years there has been an outpouring of work that applies the new economics of information to historical problems. For example, Avner Greif has applied game theoretic models to the problem of overseas trade in the medieval period and to the development of stable political institutions in twelfth- and thirteenth-century Genoa.⁸⁸ Margaret Levenstein has researched the accounting techniques adopted by American businesses during the late nineteenth century and explored the ways in which managers decided what information to collect about their enterprises’ internal operations and how the kinds of information they collected in turn affected their decision making.⁸⁹ Kenneth Snowden has demonstrated that the peculiar information problems inherent in the interregional mortgage market caused lenders to suffer repeated cycles of organizational innovation, overexpansion, and crisis that were finally ended only by the development of federal mortgage guarantees.⁹⁰ To give one last example, Peter Temin has argued that the impact of government actions during the Great Depression was determined by the “policy regime” within which people perceived them to occur. Thus open-market purchases by the Federal Reserve Bank had very different macroeconomic effects during Herbert Hoover’s presidency than precisely the same actions did under Franklin D. Roosevelt.⁹¹

All these economic historians are well read in the historical literature, and many of them have done extensive archival research. Their studies are of high quality and should be of great interest to historians working in related areas. Whether, however, it is possible to communicate this relevance over the wall that currently divides economic history from the rest of the historical profession is a matter of serious concern. Several efforts are now under way to encourage interdisciplinary discussion. For example, scholars associated with the National Bureau of Economic Research have sponsored a series of conferences to bring together economic historians, business historians, and economic theorists.⁹² Sim-

ilarly, Judith Miller of Emory University has spearheaded an effort to encourage participation by historians in the Economic History Association and economic historians in the American Historical Association. Partly as a result of Miller's efforts, EHA president Deirdre McCloskey proclaimed the theme of the association's 1997 meeting to be "interdisciplinary conversations." There are also a large number of local and regional economic history seminars that provide opportunities for economists and historians interested in a common set of problems to mix. One of the largest and best organized, the All-University of California Economic History Conference, has been promoting interdisciplinary exchanges at its twice yearly meetings since the 1970s.

Nonetheless, the struggle is an uphill one. Funding to attend national conferences is limited, so many scholars go only to one or two professional meetings a year and are unlikely to use their scarce travel dollars to cross disciplinary boundaries. At the same time, the expansion both of higher education and of the number of subdisciplines over the last quarter century has resulted in a multiplication of the number of academic books and journals being published. Scholars are finding it increasingly difficult to keep up with the literature in their own fields, let alone follow developments in other areas. Moreover, anyone who, in the last few years, has attended one of meetings devoted to bringing together scholars with different academic backgrounds will recognize that over time the various fields have developed such separate vocabularies, concerns, and research agendas that it can be painfully difficult to conduct interdisciplinary conversations.

Despite recent efforts, therefore, the organizations that cliometricians built so assiduously to promote their work during the early sixties now increasingly demarcate an intellectual ghetto. So, of course, do the many associations that the various other historical subdisciplines—from labor history to African-American and women's history to queer studies—have constructed over the past quarter century. Because, however, the cliometric revolution has largely run its course, it highlights in an especially clear way the double-edged character of these organizations. In particular, it highlights the intellectual costs that disciplinary fragmentation can entail.

NOTES

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1. Lee Benson, "Research Problems in American Political Historiography," in Mirra Komarovsky, ed., *Common Frontiers of the Social Sciences* (Glencoe, Ill., 1957), pp. 113–14. See also the essays collected in Robert P. Swierenga, ed., *Quantification in American History: Theory and Research* (New York, 1970). For a long-term perspective on American historians' relations with the social sciences, see chapter 4 by Dorothy Ross in this volume.

2. For an excellent survey of the fruits of this revolution, see Jeremy Atack and Peter Passell, *A New Economic View of American History from Colonial Times to 1940*, 2nd ed. (New York, 1994). For earlier assessments by participants and disciples, see Lance E. Davis, "'And It Will Never Be Literature': The New Economic History: A Critique," in Ralph L. Andreano, ed., *The New Economic History: Recent Papers on Methodology* (New York, 1970), pp. 67–83; Albert Fishlow and Robert W. Fogel, "Quantitative Economic History: An Interim Evaluation: Past Trends and Present Tendencies," *Journal of Economic History* 31 (Mar. 1971), pp. 15–42; and Donald N. McCloskey, "The Achievements of the Cliometric School," *Journal of Economic History* 38 (Mar. 1978), pp. 13–28.

3. Robert William Fogel, "The Specification Problem in Economic History" *Journal of Economic History* 27 (Sept. 1967), p. 284; Douglass C. North, "Economic History: Its Contribution to Economic Education, Research, and Policy," *American Economic Review* 55 (May 1965), p. 86. See also John R. Meyer and Alfred H. Conrad, "Economic Theory, Statistical Inference, and Economic History," *Journal of Economic History* 17 (Dec. 1957), pp. 524–44.

4. Guy S. Callender, "The Position of American Economic History," *American Historical Review* 19 (Oct. 1913), pp. 80–81; Edwin F. Gay, "The Tasks of Economic History," *Journal of Economic History* 1 (Dec. 1941), supplement, pp. 9–16; Steven A. Sass, *Entrepreneurial Historians and History: Leadership and Rationality in American Economic Historiography, 1940–1960* (New York, 1986), pp. 15–19; Robert William Fogel, "The Reunification of Economic History with Economic Theory," *American Economic Review* 55 (May 1965), pp. 94–95. For an excellent example of the "compromise" position as it was worked out in British economic history, see J. H. Clapham, "The Study of Economic History," in N. B. Harte, ed., *The Study of Economic History: Collected Inaugural Lectures, 1893–1970* (London, 1971), pp. 57–70.

5. Sass, *Entrepreneurial Historians and History*, p. 15; Arthur H. Cole, "Economic History in the United States: Formative Years of a Discipline," *Journal of Economic History* 28 (Dec. 1968), pp. 558–59.

6. Callender, "The Position of Economic History," p. 80.

7. I sampled one out of every five volumes of the *American Economic Review* and *Journal of Political Economy* and two out of every five of the slimmer *Quarterly Journal of Economics*.

8. Callender, "The Position of Economic History," p. 80.

9. Sass, *Entrepreneurial Historians and History*, pp. 24–26. Lack of a clear disciplinary home was a common refrain among British economic historians as well. See the inaugural addresses given by British chair holders in economic history. They are collected in Harte, ed., *The Study of Economic History*. Many British universities later coped with this problem by setting up separate departments of economic history.

10. Abbott Payson Usher, "The Application of the Quantitative Method to Economic History," *Journal of Political Economy* 40 (Apr. 1932), pp. 186–209; Herbert Heaton, "Recent Developments in Economic History," *American Historical Review* 47 (July 1942), pp. 727–46; Cole, "Economic History in the United States," pp. 571–79.

11. Cole, "Economic History in the United States," pp. 573–75, 579; Sass, *Entrepreneurial Historians and History*, pp. 29–34.

12. Gay, "The Tasks of Economic History," p. 9; Cole, "Economic History in the United

States," pp. 559, 560–61, 565–67. Cole, a student of Gay's, worked on the American woolen industry, but even here he felt the influence of European scholarship. As Cole put it, "It was perhaps a reflection of Gay's thorough training in German materials that he was annoyed with me for failing to find evidences of the appearance of a putting-out stage" (p. 567). This is not to say, however, that there were no American antecedents for these intellectual movements. For example, the experience of American scholars (including Gay) who collected statistics for government planning agencies during World War I helped spur interest in quantification.

13. John U. Nef, "The Responsibility of Economic Historians," *Journal of Economic History* 1 (Dec. 1941), supplement, p. 7. He was quoting Herbert Heaton.

14. Herbert Heaton, "The Early History of the Economic History Association," *Journal of Economic History* 1 (Dec. 1941), supplement, p. 107.

15. *Ibid.*, p. 107.

16. *Ibid.*, pp. 107–9; and Herbert Heaton, "Twenty-Five Years of the Economic History Association: A Reflective Evaluation," *Journal of Economic History* 25 (Dec. 1965), p. 470.

17. Sass, *Entrepreneurial Historians and History*, pp. 54–59.

18. *Ibid.*, pp. 53–106; Hugh G. J. Aitken, "Entrepreneurial Research: The History of an Intellectual Innovation," in Aitken, ed., *Explorations in Enterprise* (Cambridge, 1965), pp. 3–19; Arthur H. Cole, "The Committee on Research in Economic History: An Historical Sketch," *Journal of Economic History* 30 (Dec. 1970), pp. 723–41.

19. This search for theory often took the form of written scholarly debate in the pages of the center's in-house journal, *Explorations in Entrepreneurial History*. For an excellent analysis of the process by which participants turned to Parsonian sociology, see Sass, *Entrepreneurial Historians and History*, pp. 107–223. For an analysis of the utility of this body of theory from someone associated for a time with the center, see Louis Galambos, "Parsonian Sociology and Post-Progressive History," *Social Science Quarterly* 50 (June 1969), pp. 25–45. I am also indebted for this account to Louis Cain, E-mail communication of Jan. 18, 1996.

20. "An Interview with Lance Davis," *Newsletter of the Cliometric Society* 5 (Feb. 1990), pp. 9–10; Samuel H. Williamson, "The History of Cliometrics," in *Two Pioneers of Cliometrics: Robert W. Fogel and Douglass C. North* (Oxford, Ohio, 1994), pp. 119, 124–26.

21. "An Interview with Jonathan R. T. Hughes," *Newsletter of the Cliometric Society* 6 (Oct. 1991), p. 24.

22. "An Interview with Robert W. Fogel," *Newsletter of the Cliometric Society* 5 (July 1990), p. 6.

23. "An 'Interview' with Robert E. Gallman," *Newsletter of the Cliometric Society* 7 (Feb. 1992), pp. 5–6.

24. See Stephen Haber, "Introduction: Economic Growth and Latin American Economic Historiography," in Haber, ed., *How Latin America Fell Behind: Essays on the Economic History of Brazil and Mexico* (Stanford, 1997), pp. 5–9.

25. "An Interview with William N. Parker," *Newsletter of the Cliometric Society* 6 (July 1991), pp. 23–24.

26. Douglass C. North, "Quantitative Research in American Economic History," *American Economic Review* 53 (Mar. 1963), pp. 128–29.

27. North, "Economic History," p. 87. Similarly, Lance E. Davis, Jonathan R. T. Hughes, and Stanley Reiter ended their milder plea for quantitative economic history with the warning: "if the discipline chooses to remain completely in the literary tradition, we can see small hope for anything but a continual rehashing of the already existing sources and a continuation of the century-long cleavage between economics and economic history."

"Aspects of Quantitative Research in Economic History," *Journal of Economic History* 20 (Dec. 1960), p. 547.

28. North, "Economic History," p. 90.

29. "An Interview with Robert W. Fogel," p. 28.

30. Heaton, "Recent Developments in Economic History," p. 735.

31. E. A. J. Johnson, "New Tools for the Economic Historian," *Journal of Economic History* 1 (Dec. 1941), supplement, p. 38. Fogel recognized that traditional economic historians had done quantitative work, but he claimed that they "limited themselves primarily to the presentation, in more or less original form, of data found in standard historical sources." See his comment on papers by Peter Temin, Albert Fishlow, and Roger L. Ransom, "Discussion," *American Economic Review* 54 (May 1964), p. 378. See also Lance Davis, "Professor Fogel and the New Economic History," *Economic History Review*, 2nd ser., 19 (Dec. 1966), p. 658.

32. "An Interview with John Meyer," *Newsletter of the Cliometric Society* 10 (Feb. 1995), p. 4. The Conrad and Meyer paper is generally regarded as the first major cliometrics paper.

33. "An 'Interview' with Robert E. Gallman," p. 4. These meetings preceded the Purdue conferences and were important in attracting attention to the earliest cliometric work.

34. Sass, *Entrepreneurial Historians and History*, p. 245; "An Interview with John Meyer," p. 22; Cole, "The Committee on Research in Economic History," p. 738; Williamson, "The History of Cliometrics," p. 116; Claudia Goldin, "Cliometrics and the Nobel," *Journal of Economic Perspectives* 9 (spring 1995), pp. 193–94.

35. "An Interview with Lance Davis," p. 7. Apparently Taylor would reject Davis's submissions out of hand, without even sending them out for review. Conversation with Davis, May 17, 1995.

36. Robert William Fogel, "A Life of Learning," *ACLS Occasional Paper*, no. 34 (1996 Charles Homer Haskins Lecture), p. 8.

37. "An 'Interview' with Robert E. Gallman," p. 5. Meyer also remarked on Redlich's hostility. See "An Interview with John Meyer," p. 5.

38. "An Interview with Douglass C. North," *Newsletter of The Cliometric Society* 8 (Oct. 1993), p. 11.

39. Goldin, "Cliometrics and the Nobel," p. 194.

40. The divisions are eloquently captured in the ambivalence with which the institutionalist Cole wrote about developments in the field during the interwar period. Although he labeled this period "The Efflorescence," his description of scholarly trends contains words like "devastating" and "misfortune." See "Economic History in the United States," pp. 571–79. In a conciliatory moment Fogel himself admitted that "the discipline of economic history was no more monolithic in the past than it is at present" and that "every one of the elements which taken together serve to define the new economic history can be found in one or another of the classics of the past." See his comment on papers by Temin, Fishlow, and Ransom, "Discussion," p. 388.

41. Peter Temin, *The Jacksonian Economy* (New York, 1969), pp. 15–22. Only one of the scholars whom Temin lists as prominent sources of the traditional view could be considered an economic historian. That was Bray Hammond. The others, Richard Hofstadter and Marvin Meyers, were mainstream historians of the consensus school, who had acquired from the progressives much of their sense of economic history.

42. Douglass C. North, *Growth and Welfare in the American Past: A New Economic History* (Englewood Cliffs, N.J., 1966), Chap. 11.

43. Robert W. Fogel, "A Quantitative Approach to the Study of Railroads in American Economic Growth: A Report of Some Preliminary Findings," *Journal of Economic History* 22

(June 1962), pp. 163–97; Fogel, *Railroads and American Economic Growth: Essays in Econometric History* (Baltimore, 1964), pp. 1–9. Goodrich advised Fogel's master's thesis on the Union Pacific Railroad. See Fogel, "A Life of Learning," pp. 5–6.

44. On this point, see also Goldin, "Cliometrics and the Nobel," p. 195.

45. Fritz Redlich, "New and Traditional Approaches to Economic History and Their Interdependence," *Journal of Economic History* 25 (Dec. 1965), p. 486.

46. Robert William Fogel, "Notes on the Social Saving Controversy," *Journal of Economic History* 39 (Mar. 1979), p. 5.

47. Louis M. Hacker, "The New Revolution in Economic History: A Review Article Based on *Railroads and Economic Growth: Essays in Econometric History* by Robert William Fogel," *Explorations in Entrepreneurial History*, 2nd ser., 3 (spring 1966), pp. 166–69. It is doubtful, however, that historians like Hacker would have formulated their views so explicitly without Fogel's demonstration that the actual cost savings of railroads over canals were so minimal.

48. See Joseph A. Schumpeter, *The Theory of Economic Development: An Inquiry into Profits, Capital, Credit, Interest, and the Business Cycle* (Cambridge, 1934).

49. Fogel, *Railroads and American Economic Growth*, pp. 14–15.

50. Parker was critical of this tendency to produce "simply a kind of hymn to what really happened." William N. Parker, "From Old to New to Old in Economic History," *Journal of Economic History* 31 (Mar. 1971), pp. 6–7.

51. Meyer has since changed his mind about the importance of entrepreneurship. See "An Interview with John Meyer," pp. 22–23.

52. Douglass C. North, *The Economic Growth of the United States, 1790–1860* (Englewood Cliffs, N.J., 1961), pp. 8, 52.

53. On this point, see Fogel, "The Reunification of Economic History with Economic Theory."

54. See for example, Lance E. Davis and Douglass C. North, *Institutional Change and American Economic Growth* (Cambridge, 1971).

55. Douglass C. North, "Beyond the New Economic History," *Journal of Economic History* 34 (Mar. 1974), p. 2. On North's transformation, see Richard C. Sutch, "Douglass North and the New Economic History," in *Two Pioneers of Cliometrics*, pp. 77–79.

56. "An Interview with Douglass C. North," p. 9.

57. See especially his *Institutions, Institutional Change, and Economic Performance* (New York, 1990). Other new economic historians who have attempted to broaden their analysis beyond the basic neoclassical model include Paul David, Peter Temin, and Gavin Wright. See, for example, David, "CLIO and the Economics of QWERTY," *American Economic Review* 75 (May 1985), pp. 205–20; Wright, *The Political Economy of the Cotton South: Households, Markets, and Wealth in the Nineteenth Century* (New York, 1978); and Temin, *Taking Your Medicine: Drug Regulation in the United States* (Cambridge, 1980). Fogel has also recently broadened his work to include, for example, the qualitative analysis of religious beliefs. See *Without Consent or Contract: The Rise and Fall of American Slavery* (New York, 1989), part 2. See, in addition, Alexander J. Field, "The Future of Economic History," in Field, ed., *The Future of Economic History* (Boston, 1987), pp. 1–41.

58. Heaton, "Twenty-Five Years of the Economic History Association," p. 472.

59. Robert Whaples, "A Quantitative History of the *Journal of Economic History* and the Cliometric Revolution," *Journal of Economic History* 51 (June 1991), pp. 291–94. Whaples defined a cliometric article as one that had both a theoretical and a quantitative dimension.

60. Alfred D. Chandler, Jr., opposed the move to transform the Business History Conference into a formal organization because he did not want to abandon the Economic

History Association to the cliometricians. His point of view did not prevail, however. Videotape of "Heritage Session," consisting of informal remarks by Harold F. Williamson, Sr., Donald Kemmerer, Alfred D. Chandler, Jr., and Wayne Broehl (reading comments from Thomas Cochran), 34th Annual Meeting of the Business History Conference, Atlanta, Ga., 1988. I am also basing this account on the recollections of Louis Cain, communicated to me in an E-mail message of Jan. 18, 1996. Members of the Business History Conference also typically subscribed to and published in the *Business History Review*, which came out of the Harvard Business School but over time came to represent the same style of business history as the conference.

61. See Haber, "Introduction."

62. George Grantham, "Cliometrics in France: A Revolution Manquée?" unpub. paper, Department of Economics, McGill University, 1994. The chair was held by C. E. Labrousse, whose brand of economic history was similar to that of quantifiers in the United States during the interwar period.

63. Richard Tilly, "Soll und Haben: Recent German Economic History and the Problem of Economic Development," *Journal of Economic History* 29 (June 1969), pp. 298–319. See also Donald N. McCloskey, "Editor's Introduction," in McCloskey, ed. *Essays on a Mature Economy: Britain after 1840* (Princeton, 1971), p. 3.

64. Mathias, "Living with the Neighbours: The Role of Economic History," in Harte, ed., *The Study of Economic History*, pp. 369–83. On the lukewarm reception of the new economic history in Great Britain, see Jonathan R. T. Hughes, "Is the New Economic History an Export Product?" R. M. Hartwell, "Is the New Economic History an Export Product? A Comment on J. R. T. Hughes," Barry Supple, "Can the New Economic History Become an Import Substitute?" and R. C. O. Matthews, "The New Economic History in Britain: A Comment on the Papers by Hughes, Hartwell, and Supple," in McCloskey, ed., *Essays on a Mature Economy*, pp. 401–33. See also McCloskey, "Editor's Introduction," p. 3.

65. See Roderick Floud, ed., *Essays in Quantitative Economic History* (Oxford, 1974); Floud, *An Introduction to Quantitative Methods for Historians* (London, 1973); Maurice Lévy-Leboyer and François Bourguignon, *L'économie française au XIX^e siècle: Analyse macro-économique* (Paris, 1985); Grantham, "Cliometrics in France."

66. There are now also sixteen members from Asia (including Russia) and seven from Australia and New Zealand. Cliometric Society membership figures.

67. I am using Robert Whaples's definition for this calculation. See note 59 above and Whaples, "A Quantitative History of the *Journal of Economic History* and the Cliometric Revolution." On the growth of cliometric work in Britain, see also Grantham, "Cliometrics in France."

68. Grantham, "Cliometrics in France"; conversation with Tim Hatton of Essex University, who is to be one of the editors of the new journal, Sept. 8, 1995.

69. See Field, "The Future of Economic History," pp. 1–2, 14–16.

70. For a discussion of the mood of the historical profession during this "Era of No Hard Feelings," see Peter Novick, *That Noble Dream: The "Objectivity Question" and the American Historical Profession* (New York, 1988), pp. 321–411. See also John Higham, *History: Professional Scholarship in America* (Englewood Cliffs, N.J., 1965), pp. 132–44, 212–32.

71. Novick, *That Noble Dream*, pp. 424–26, 437–38.

72. *Ibid.*, Chap. 14.

73. Robert William Fogel and Stanley L. Engerman, *Time on the Cross: The Economics of American Negro Slavery* (Boston, 1974), pp. 4–6.

74. Oscar Handlin, "The Capacity of Quantitative History," *Perspectives in American History* 9 (1975), pp. 7–8. On this point, see also Norman R. Yetman, "The Rise and Fall

of *Time on the Cross*," *Reviews in American History* 4 (June 1976), pp. 196–97; and Kenneth M. Stampp, "Introduction: A Humanistic Perspective," in Paul A. David et al., *Reckoning with Slavery: A Critical Study in the Quantitative History of American Negro Slavery* (New York, 1976), pp. 9–10.

75. Fogel and Engerman, *Time on the Cross*, pp. 4–6.

76. *Ibid.*, pp. 4, 7. On this point, see C. Vann Woodward, "The Jolly Institution," *New York Review of Books* 21 (May 2, 1974), p. 3. For notable exceptions, see Handlin, "The Capacity of Quantitative History"; and the review of *Time on the Cross* by Frank B. Tipton, Jr., and Clarence E. Walker, published in *History and Theory* 14 (Feb. 1975), pp. 91–121.

77. Some of the most important criticism has been collected in David et al., *Reckoning with Slavery*.

78. *Ibid.*, pp. vi–vii.

79. Stampp, "Introduction," in *ibid.*, pp. 1–2.

80. Yetman, "The Rise and Fall of *Time on the Cross*," p. 202. Herbert G. Gutman worried about this effect as well. See *Slavery and the Numbers Game: A Critique of Time on the Cross* (Urbana, 1975), p. 13.

81. This can easily be confirmed by a quick glance at Attack and Passell, *A New Economic View of American History*, chaps. 11 and 12. The debate also spilled over to stimulate new work on the postbellum South.

82. Thomas L. Haskell, "The True & Tragical History of 'Time on the Cross,'" *New York Review of Books* 22 (Oct. 2, 1975), p. 34.

83. John Kenneth Galbraith, *The Great Crash*, 1929 (Boston, 1955); Milton Friedman and Anna Jacobson Schwartz, *The Great Contraction, 1929–1933* (Princeton, 1965); Peter Temin, *Did Monetary Forces Cause the Great Depression?* (New York, 1976); Barry Eichengreen, *Golden Fetters: The Gold Standard and the Great Depression, 1919–1939* (New York, 1992); Peter Temin, *Lessons from the Great Depression* (Cambridge, 1989). For a survey of the voluminous article literature on this subject, see Attack and Passell, *A New Economic View of American History*, chap. 21.

84. See, for example, Robert W. Fogel et al., "Secular Changes in American and British Stature and Nutrition," *Journal of Interdisciplinary History* 14 (autumn 1983), pp. 445–81; and Fogel, "Nutrition and the Decline in Mortality Since 1700: Some Preliminary Findings," in Stanley L. Engerman and Robert E. Gallman, eds., *Long-Term Factors in American Economic Growth* (Chicago, 1986), pp. 439–555. For a survey of this literature, see Richard H. Steckel, "Stature and the Standard of Living," *Journal of Economic Literature* 33 (Dec. 1995), pp. 1903–40.

85. The whole literature on the mentalité of farmers is riddled with such errors. See, for example, Michael Merrill, "Cash Is Good to Eat: Self-Sufficiency and Exchange in the Rural Economy of the United States," *Radical History Review* 4 (winter 1977), pp. 42–71; and James A. Henretta, "Families and Farms: Mentalité in Pre-Industrial America," *William and Mary Quarterly* 35 (Jan. 1978), pp. 3–32. Although Christopher Clark has read some economic history, he does not completely escape the errors of his predecessors. See *The Roots of Rural Capitalism: Western Massachusetts, 1780–1860* (Ithaca, 1990).

86. See John E. Toews, "Intellectual History after the Linguistic Turn: The Autonomy of Meaning and the Irreducibility of Experience," *American Historical Review* 92 (Oct. 1987), 879–907. The quote is from p. 882. On this point, see also chapter 4 by Dorothy Ross in this volume.

87. For an introduction to this literature, see Daniel M. G. Raff and Peter Temin, "Business History and Recent Economic Theory: Imperfect Information, Incentives, and the Internal Organization of Firms," in Temin, ed., *Inside the Business Enterprise: Historical*

Perspectives on the Use of Information (Chicago, 1991), pp. 7–35. See also Oliver E. Williamson, *The Economic Institutions of Capitalism: Firms, Markets, Relational Contracting* (New York, 1985); Jean Tirole, *The Theory of Industrial Organization* (Cambridge, 1988); John Eatwell, Murray Milgate, and Peter Newman, eds., *The New Palgrave: Allocation, Information, and Markets* (New York, 1989); Richard Schmalensee and Robert Willig, eds., *Handbook of Industrial Organization* (Amsterdam, 1989); and Thrainn Eggertsson, *Economic Behavior and Institutions* (Cambridge, 1990).

88. Avner Greif, "Reputation and Coalitions in Medieval Trade: Evidence on the Maghribi Traders," *Journal of Economic History* 49 (Dec. 1989), pp. 857–82; and Greif, "On the Political Foundations of the Late Medieval Commercial Revolution: Genoa During the Twelfth and Thirteenth Centuries," *Journal of Economic History* 54 (June 1994), pp. 271–87.

89. Margaret Levenstein, "The Use of Cost Measures: The Dow Chemical Company, 1890–1914," in Temin, ed., *Inside the Business Enterprise*, pp. 71–112; and Levenstein, *Accounting for Growth: Competition, Information Systems, and the Creation of the Large Corporation* (Stanford, in press). See also H. Thomas Johnson and Robert S. Kaplan, *Relevance Lost: The Rise and Fall of Management Accounting* (Boston, 1987).

90. Kenneth A. Snowden, "The Evolution of Interregional Mortgage Lending Channels, 1870–1940: The Life Insurance–Mortgage Company Connection," in Naomi R. Lamoreaux and Daniel M. G. Raff, eds., *Coordination and Information: Historical Perspectives on the Organization of Enterprise* (Chicago, 1995), pp. 209–47.

91. Temin, *Lessons from the Great Depression*.

92. The proceedings of the first two conferences have been published in Temin, ed., *Inside the Business Enterprise*, and Lamoreaux and Raff, eds., *Coordination and Information*. Papers given at a third conference on learning by firms, organizations, and nations will appear in a forthcoming volume edited by Lamoreaux, Raff, and Temin.

The New and Newer Histories: Social Theory and Historiography in an American Key

DOROTHY ROSS

DURING THE TWENTIETH CENTURY, historians in Europe and the United States have repeatedly revised their historical programs to make greater use of social theory. Beginning early in the century with the American Progressives' "New History" and the French *Annales*, there has been a succession of "new histories" based on alliance with the social sciences. Instead of writing narrative accounts of political events, the new historians used social theory to analyze the social and economic forces within and structures beneath the course of national politics. Using the *Annales* as his prime example, Georg Iggers links the rise of new history and its more structural understanding of historical process to the decline of belief in progress around the time of the First World War. "Exactly because the societies and cultures of the past are no longer seen as stages in a linear progression, they are now viewed not merely diachronically but also synchronically as structures possessing a degree of integrity and stability in time." As such, they invited analysis rather than narrative, and attention to collectivities and their material, social, and cultural conditions rather than individual actions.¹

These new histories of the twentieth century thus stand at the intersection of two larger histories. One is the history of historicism, defined broadly as that "historical-mindedness" which began in the eighteenth century: a recognition of the qualitative difference, the "otherness," of the past, which mandated that human affairs be understood historically.² While abandoning or attenuating the conception of linear progress and the focus on political events that gave shape to nineteenth-century historicism, the new histories of the twentieth century retained the grounding in temporality.³

The new histories also belong to the history of the social sciences, studies that grew out of the Enlightenment effort to understand modernity. In their belief that the West had embarked on a novel course of historical development that was still unfolding, early social scientists shared the historicism of the eighteenth century. But unlike the historians, their focus was on the social and economic dimensions of civil society that modernity disclosed. Using analogues of scientific method, they produced social theories rather than political narratives.⁴ During the nineteenth century history and the social sciences diverged and intertwined in a number of ways, although it was not until the twentieth century that a

succession of new historians made a concerted effort to use social theory in their practice.

At first glance, the relation between historiography and social theory in the United States seems to follow a different path from that Iggers laid out. The New History that was announced in the United States in 1912 did not abandon, but emphasized, the liberal narrative of Western progress. When American historians finally turned to analytic, structural history and a full alliance with the social sciences in the decade after World War II, it was during a moment of American triumphalism. On closer examination, however, the United States proves to be a variant, rather than an exception. Both historicism and social science have had somewhat different histories in the United States, producing differences in the character and timing of anxieties about the course of history and the uses of social theory. In the American case, too, new historians used social theory to stabilize an increasingly uncertain narrative of Western history. The newer histories that make use of postmodern theories both extend and alter that story.

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What accounts for the American variation on this Western theme is historical consciousness, shaped in the United States, as it was in the other countries of Europe, by the understanding of national history. The writers, politicians, and clergymen who constructed that national self-understanding in the decades after the American Revolution located the United States within the story of Western progress, a liberal story of growing commercial development, representative political institutions based on democratic consent, and the advance and diffusion of knowledge, processes that were projected to remake the entire world. They seated world progress not in Europe, where a class-ridden feudal past and industrial future distorted history, but in the American nation. The special place of the United States in this story was attributed in part to favorable historical conditions that allowed it to form a New World antithetical to the Old: the heritage of Anglo-Saxon institutions, the republican frame of government, the continent of uncultivated land, the opportunity offered by a free market of small producers. But specialness derived fundamentally from divine favor, a favor that began with the Puritan mission to New England and was sealed in the Revolution and Constitution. The country's unique foundation located it in millennial as well as historical time, freeing it from the ills of Europe and guaranteeing it an ideal future, exemplary for the world. In this view, American progress would be a quantitative multiplication and elaboration of the country's founding institutions, not a process of qualitative change. George Bancroft gave this exceptionalist historical consciousness its most popular form in the nineteenth century, while his contemporaries J. L. Motley and William H. Prescott set the pattern for American historians of Europe, who found there histories of decline that proved the rule of American progress. American exceptionalism was thus one variant among many nationalist versions of special destiny derived from a Christian heritage.⁵

During the Gilded Age, roughly from the late 1870s to late 1890s, the weakening of religious belief and the industrial transformation of society called American

exceptionalism into question. The Gilded Age was also the period in which history and the new social sciences established disciplinary identities in the universities. The historians wanted to separate history from its divine background and turn it into an historical science on the Rankean model. The central figures in the movement to professionalize history, however, like Andrew Dickson White, John W. Burgess, and Herbert Baxter Adams, also believed that history and political science were part of the same large field. Their double enterprise of historico-politics was linked by a common task: amidst rapid industrial development, fierce social conflict, and widespread political corruption, they wanted to strengthen established historical principles so as to guide political action in a conservative direction. They sought this structural support in political principles and institutions: Adams's tracing of Teutonic "germs" from old to New England communities was one contribution to this program, as was the importation of Germanic conceptions of the "State."

At the same time, however, they were cognizant of the economic and social upheavals around them and of the new social science disciplines competing for academic space; they opened their field outward and laid the groundwork for the New History. They often spoke of the historical and political sciences in the plural and took under their wings historical jurisprudence, economics, and sociology. Adams's students, among them Frederick Jackson Turner, quickly developed social and economic dimensions in their work. At Columbia, the interdisciplinary environment Burgess established was the seedbed for James Harvey Robinson's announcement of a New History in 1912 and for Charles Beard's economic interpretation of history.⁶

Taking over from historico-politics the desire to link historical knowledge to present politics and to widen attention to economic and social history, this younger generation also had a deeper appreciation of historicism. The Gilded Age crisis had opened the way to a full recognition of the difference of the American past and the country's dependence on the contingent forces of history. To secure the ideal American future, the New Historians fully attached American history to Western liberal history and its progressive motors of capitalism, democracy, and science. The New History turned to the social sciences because those studies show us not accidental events, Robinson said, but "the general trend of development and progress."⁷ European history was no longer seen as a realm of failure that proved American success, but as a realm of progress continuous with that of the United States and moving toward a common goal.⁸ Exceptionalism was retained by placing the United States at the forefront of the movement and by casting progress in American shapes.

Turner was a transitional figure in this New History. Deeply committed to the old exceptionalist ideal, he located the source of American democracy in the vanishing frontier rather than in the new industrial process. He then tried to translate the frontier thesis into sectional analysis, in the vain hope that the continued diversity of sections and the democratic character of the American West would provide a continuing basis for American democracy. Geography and comparative geography were tools in this project and had some influence on

the school of Western history founded on his work but never became a major source of social-theoretical interest in American historiography.⁹ Geography, as *Annales* history confirms, is best suited to provide continuity, and in American historiography political principles continued to serve that purpose until after World War II.

In contrast, Robinson and Beard were driven by their liberal reformist politics to look forward: the ideal American democracy had yet to be achieved, and they were impatient with the slow pace of reform. Their models were the progressive European evolutionary theories of the nineteenth century and the adaptations of them American social scientists had already begun to make in the 1890s. In *The New History* Robinson borrowed Thorstein Veblen's evolutionary concept of institutions as "habits of thought," but his major focus was on social psychology. An intellectual historian and latter-day *philosophe*, he believed that advances in science and knowledge were the chief factor in progress. The social psychologies of James Mark Baldwin, Gabriel Tarde, and Sigmund Freud explained the irrationality of the masses of humanity, the inertial force that progress must overcome. Robinson never inserted such social psychological analysis into his textbooks or articles but elaborated it only in his popular book *The Mind in the Making* (1921), where it remained programmatic.¹⁰

Beard's economic interpretation of history was by far the most influential form of the New History. He learned it from his exposure to socialism in England and from his Columbia colleague, the historical economist E. R. A. Seligman, who had transformed Marx's historical materialism into a liberal theory of capitalist progress. Beard's focus was on the economic basis of politics and the construction of American democracy. But Seligman's theory did not offer much guidance on the specific links between economic conditions, politics, and ideas. Beard chronicled the conflict between social-economic classes, but dealt with them most often as economic interest groups rather than structural classes.¹¹ Politics and ideas were generally treated as derivative of economic and social "forces" and perpetually lagging behind technological and industrial advance. This view of historical change, formalized by the sociologist William Ogburn in the 1920s as "cultural lag," ran all through Progressive social science and historiography. It configured social conflict not as structural contradiction but as a partiality in historical advance that progress would resolve.¹²

Still, lagging progress exposed the dependence of the American ideal on the contingencies of history. After World War I and the conservative reaction of the 1920s, the New Historians' anxiety was palpable. It was reflected in Robinson's stress on irrationality in *The Mind in the Making* and in the textbook he titled *The Ordeal of Civilization* (1926). In the companion volume by Charles and Mary Beard, *The Rise of American Civilization*, progress operated as the regulative principle of the narrative, but it was now a question that had to be explicitly asked and answered. The uncertainties of history drove a wedge between the New History and the social sciences. During the Progressive era, Robinson and Beard had based the historical alliance with the social sciences on a common genetic viewpoint, and a younger generation of historians continued that project during the

interwar decades. Social scientists, however, soon broke with historical and evolutionary theory in order to form instrumental sciences that would enable them to predict and control the uncertain course of human affairs.¹³

It was not until the decades after World War II that American social science and historiography joined again, this time around a more structural, analytic model of historiography. During this period the social sciences were in command of growing resources and intellectual authority in academia, government, and popular culture. Over the course of the 1930s and 1940s they had absorbed new influences from European social theory. Talcott Parsons brought into American social theory a Durkheimian sense of the reality of social norms. The work of Max Weber also had a major impact on the structural understanding of society. Although Marx was largely proscribed in American universities, his work nonetheless influenced social thinkers in and out of academia. American social science thus greatly expanded its conceptual repertoire.

At the same time, it shifted from a focus on socializing individuals to a concern for integrated social systems. Society was still presumed to consist of aggregated individuals, but the statistical analysis of collective behaviors and the functional constituents of society that ruled behavior became the focus of analysis. System norms were assumed to achieve social cohesion and equilibrium. In politics, too, American democracy was configured as a pluralist system of competing interest groups that tended toward an equilibrium of justice and order. These functionalist theories incorporated the description of modernity found in earlier evolutionary theories, but functionalism assumed a static society, removed from history. It often identified the social systems actually at work in contemporary American society with the exceptionalist ideal. Older theories of liberal progress, recast as modernization theory, were applied to the world outside the United States and measured the distance still to be traversed to achieve the American norm.¹⁴

The social sciences thus participated in the construction of a newly static historical consciousness. During the decade after World War II, the country was experiencing the "American Moment" of the "American Century," when the United States seemed already to stand at the summit of world power and already to embody the values its exceptionalist history promised.¹⁵ At the same time, however, Cold War abroad and McCarthyism at home created a new sense of anxiety about the exceptionalist triumph. To some critical intellectuals, the dominant position of the United States in the world quickly raised questions about the limits of American power and the American example. The possibility that America's unique consensus had disabled the country from effectively playing its leading role in the world—a possibility soon reinforced by widespread criticism of the United States—threatened the universal consummation of exceptionalist history.¹⁶ It was in this context that the historians Richard Hofstadter, H. Stuart Hughes, David Potter, and Edward N. Saveth, among others, called for a new alliance with the social sciences.¹⁷ The static structuralism of the postwar/Cold War social sciences, with their sense of liberal progress achieved, promised to stabilize the disorientation in time of a less than perfect triumph.

The social sciences at the same time promised relief from historians' epistemological problems. In the 1930s Beard and Carl Becker led historians into a debate on whether the historian could ever reach objective knowledge of history. Much of the profession eventually took a compromise position, concluding that certainty was possible with regard to facts but that interpretation was necessarily subjective.¹⁸ Recognizing that history was at bottom an imaginative "representation of the human situation," Hofstadter turned to social science concepts as a way of deepening the historical imagination. Potter, on the other hand, believed social science theory would remedy "the historian's lack of systematic procedure in the practice of generalization," making history more scientific.¹⁹

One consequence of both disorientation in time and the turn to social science was the demotion of narrative. The books written by these postwar/Cold War historians were what Hofstadter called "the new genre of analytical history . . . part narrative, part personal essay, part systematic empirical inquiry, part speculative philosophy."²⁰ Analytical history is framed by the historian's argument; the chronological time of its story can be interrupted or obscured to fit the purposes of argument.

Another consequence was the construction of an ironic version of exceptionalist history that reflected contemporary postwar/Cold War concerns.²¹ Social theory helped to conceptualize American failings as inevitable aspects of success. Ironic historians argued that the achievement of egalitarian democracy led to a harmonious pluralist order, but this very success and the consequent absence of real ideological conflict and debate led to inflated expectations and a conformist, absolutist mentality. Drawing on Marxist influences of the 1930s that had criticized the monolithic liberalism of American society,²² this analysis centered after the war in Tocqueville's consensual understanding of American society. As Wilfred McClay has shown, a host of social scientists in the 1950s from Erich Fromm to David Riesman were finding a "soft" totalitarianism in the United States parallel to the "hard" totalitarianism attributed to Nazi Germany and the Soviet Union, an idea rooted in the critique of atomized mass society that had originated with conservative thinkers after the French Revolution, including Tocqueville. The theory was revived by the Frankfurt critics of modern society and culture, many of whom had emigrated to the United States, and then taken up by American social theorists.²³ This theoretical apparatus allowed American historians to configure American vices as the unfortunate product of consensual virtues.

The social sciences were also used extensively in historical analysis. Historians paid particular attention to concepts like status, role, culture, and personality—theories of the middle range that could specify the social, economic, and psychological relations left inchoate by the New History. These concepts were understood by many American historians to operate within a liberal functionalist theoretical framework.²⁴ Status anxiety, deviance, relative deprivation, and a host of psychological disorders defined the tensions emerging from society understood in a functionalist way. Static "social strains" replaced progressive "cultural lag" as a focus of historical/social scientific explanation.²⁵ The irrational, dysfunc-

tional processes of history were also important for American historians of Europe, who looked to European theorists like Freud and Pareto. As Leonard Krieger has pointed out, postwar historians of Europe, continuing on a well-worn American path, were attracted to the lost causes, the failed rationality, and the reactionary episodes that marked a counterpoint to the larger story of Western and American progress.²⁶

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The postwar/Cold War alliance of historiography and the social sciences around an ironic revision of American exceptionalism and its middle-range conceptual apparatus turned out to be unstable. It became the opening wedge in a proliferating series of historiographical programs making use of social science and social theory.

By the late 1950s a number of political and economic historians began to argue that it was scientific method that produced novel findings and reliable generalizations, not borrowed concepts. Although they made use of theory, they chiefly urged that historians adopt scientific methods, particularly quantitative methods. They launched a new social history “from the bottom up” that aimed to correct the impressionistic, overgeneralized stories previous historians told by using large data sets and statistical correlations.²⁷

That history was no sooner begun, however, when the political conflict of the 1960s created new historiographical energies and directions. The concatenation of the civil rights movement, the war in Vietnam, youth rebellion, and the women’s movement decisively ended the “American Moment” and its consensual explanation of American virtues and vices. What has been called the “New Left” drew into political debate and then into the historical profession a range of radical views, based in liberal democratic, populist, Marxist, and feminist traditions as well as in contemporary radical movements. It produced a social-cultural history that focused on the “inarticulate,” the working class, racial minorities, and women, those who had been marginalized in American history and left out of its historiography.²⁸ Both these new social histories “from the bottom up” were heavily influenced by the achievements of European historiography, particularly the social-cultural history of the *Annales* group in France and the English historians associated with *Past and Present* and the History Workshop, themselves influenced by Marxism and the *Annales*.²⁹ And fueling all these political and historiographical trends was the changing composition of the profession, as the postwar democratization of higher education opened a historical vocation to men and—after the mid-1960s—women from a wider spectrum of American society.³⁰

Many varieties of historiography flowed from these influences. I would like to look at three variants of new history that developed in these post-sixties decades: the social science history that formed in the mid-1970s around the Social Science History Association (SSHA), the historiography that has made use of modernization theory, and the social-cultural history of marginalized groups that was influenced by the activism of the 1960s. All three categories overlap, and I can only

touch on them here, but I will try to suggest some of the characteristic ways these American new histories used social theory and how those uses have changed in recent years. All three set out to remake American historiography on terms suggested by social theory; all have enriched historiography, but none has succeeded in its imperialist ambition.

The movement for social science history was spearheaded by a group of historians in the late 1950s who responded to the instability of historical interpretation and the heated ideological climate of the Cold War by trying to make history into a science. The ambitions of these pioneers varied from William Aydelotte's modest desire to improve the way historians generalize to Lee Benson's effort to turn history into a science that generates general laws of human behavior.³¹ They were joined by pioneer economic historians like Robert Fogel and Stanley Engerman, and in the late 1960s and 1970s by younger historians who were drawn into social science methods through their work on new social history topics, such as studies of voting behavior and demography. Their model of science was most often the empirical, behaviorist, quantitative social science practiced in the United States.³²

Some of the social scientists who joined the SSHA in the mid-1970s shared the historians' scientific aspirations, but most had a very different agenda: while retaining the goal of a generalizing science, they wanted to move beyond a narrow positivism in social science and import some of the hermeneutic understanding and contextual richness of historiography. Charles Tilly, influenced by the *Annales*, and Theda Skocpol, influenced by Barrington Moore, along with their students and allies were early participants, as were scholars of historical demography and the sociology of the family. The social science participants were chiefly political scientists, sociologists, and economists; a small group of anthropologists appeared but virtually no psychologists.³³

The social science history that resulted thus represented a number of theoretical strains, but quantitative American social science predominated. One of its most characteristic products were the voting-behavior studies that developed an ethnocultural interpretation of American politics. Like the *Annales* "serial history," these studies used numerical series to find continuous or changing patterns over time. However, the *Annalistes* took their patterns as clues to a qualitative analysis of underlying social structural conditions, while the behaviorist historians of voting submitted their numbers to statistical analysis in the hope of producing a causal account of political behavior; they largely ignored the structural features of American politics and society that shaped both political behavior and ethnocultural identity.³⁴

During the 1980s, the scientific fervor of the social science history program receded, the victim of powerful critiques of the results of quantitative history and the broad attack on positivism.³⁵ At the SSHA, the balance between historians and social scientists has shifted: in recent years social scientists have come to comprise 60 percent of the program participants.³⁶ Among social scientists the interest in history has grown, but those committed to historicism are still marginalized in their own disciplines, making the SSHA a welcome venue. The reverse

is true among historians: committed scientism has declined, while an eclectic interest in social theory is welcomed throughout the discipline.³⁷ Moreover, the array of interests represented in the SSHA has broadened. Gender has become an important area of interest, as has culture, and even narrative—the enemy against which the association originally formed—has established a beachhead.³⁸

As a result, those involved in SSHA's original aims tend to be disappointed, whether historians devoted to the original quantitative scientific program or social scientists who wanted to transform the social sciences into genuinely historicist disciplines.³⁹ The SSHA nonetheless remains one of the few forums where historians, sociologists, political scientists, and historical economists can listen to one another on topics of overlapping—if not quite mutual—interest. The joint venue also attracts European scholars. *Social Science History* now prints articles that self-consciously test the power of social theories in historical contexts, often concluding that the theories do not adequately capture the complexity of history. That exercise can be illuminating, but it is a long way from the original effort to reconstitute American historiography.

Another kind of new history emerged from efforts to use modernization theory as the narrative and analytical spine of American historiography. Modernization theory descends from ideas of liberal progress that have been powerful since the eighteenth century and from the sociological theories of Ferdinand Tönnies and Max Weber. In the version formulated by 1950s American social scientists, modernization was understood as synonymous with Westernization and, particularly in the United States, Americanization; it was designed to provide a counterideology to Marxism that would enlist the “Third World.” It cast economic development as the prime motor of progress, to which were linked changes in personality and politics. Inscripting the structural-functionalist assumptions of 1950s sociology, it tended to view modernization as an integrated, deterministic process but allowed for failure, particularly through the semiautonomous sphere of politics.⁴⁰

Modernization theory provided a social-theoretical replacement for discredited ideas of progress, but its ideological use, reductionism, and historical determinism made historians wary of it from the start. Few historical studies have in fact openly claimed modernization as the theoretical basis of their work.⁴¹ Perhaps the most influential instance of modernization theory in American historiography was Robert Wiebe's *The Search for Order, 1877–1920*, published in 1967. Applying the theory to the developing United States, Wiebe analyzed the transformation of a decentralized, agrarian-commercial society constructed around “island communities” into a nationalized, industrial capitalist, urban, bureaucratic society.⁴² While his prose sometimes hinted at distress over the impersonality of this process, what he wanted was a smoothly running, integrated bureaucratic order of the kind theorists imagined to exist in the postwar United States. Wiebe's emphasis was on the disorder of the transitional process: by 1920 the decisive turn had been taken, but there were still only “separate bureaucracies, barely joined in some areas, openly in conflict elsewhere.”⁴³ What almost all readers of Wiebe failed to notice was that after the upheavals of the late 1960s, he

lost faith in the ability of the modernization process to achieve unassisted a harmonious bureaucratic order in modern America. Instead he attempted to construct a uniquely American order that was only partially modernized, but that had, in exceptionalist fashion, turned persisting conflict into harmony.⁴⁴

The Search for Order was nonetheless a major starting point for the attempt to formulate an "organizational synthesis" of modern American history around the formation of large-scale bureaucratic organizations. Drawing on Weber, the new political science of the American state, and Alfred Chandler's pioneer work in business history, it has attempted to draw together the expanding historiography of professions, business corporations, and the intersection of state and private institutions in United States political economy. It is not clear that these historiographies, written by both liberal modernists and left critics of "corporate liberalism," constitute a "synthesis." One of its principal architects, Louis Galambos, notes that it often ignores crucial issues of conflict, power, and the distribution of resources. He also carefully disconnects his discussion of modern organizations from modernization as a necessary process. The "organizational synthesis" nonetheless assumes such a single, interconnected process, while historicizing the different forms that it takes in different locales.⁴⁵

If organizations have not provided a new master narrative of American history, modernization continues to reappear, in part as a foil against which more complex historical accounts are written, in part as the narrative line around which stories are silently told. It also reappeared with its valences reversed. Although modernization theory was resolutely progressive, it incorporated the theory of traditional society as *gemeinschaft* and thus could be turned to express ambivalence about modernity.⁴⁶ Numerous studies in American history, often elegaic in tone, chart the movement from *gemeinschaft* to *gesellschaft*, starting with the early social histories of New England communities and stretching through the twentieth century. New Left historians of working-class and popular culture, lamenting the loss of community, have found this framework particularly congenial.⁴⁷ In part as a reaction to the historiography of community, social and economic historians have begun to reexamine the early development of the capitalist market and to reaffirm the progressive course of capitalism, a line of analysis that establishes ideological as well as analytical links to modernization theory and which may take on new life with the resurgence of market economics.⁴⁸

Finally I want to discuss the social-cultural historians who were energized by the activism of the 1960s and have hoped to rewrite history on the basis of race, gender, class, and ethnicity. Rebelling against the consensus historiography of the postwar/Cold War era, they argued that workers, immigrants, racial minorities, and women had resisted domination and maintained their own group identities. Among the leaders of this compound movement, a number came from backgrounds that had exposed them to the orthodox Marxism that had survived in the United States and to Marxist theory. Rejecting orthodox Marxism but remaining attuned to Marxist theory, they and others from different backgrounds were then influenced by the broader antiauthoritarian, antiracist, and feminist currents of the 1960s.⁴⁹ As a result, Marxist theory was modified by an eclectic

and culturally oriented mix of social theory: revisionist Marxism, particularly Gramsci and the English historians Raymond Williams and E. P. Thompson; the symbolic anthropology of Clifford Geertz, Victor Turner, and others; and most recently poststructural literary theories. The implications of this last influence—poststructural literary theory—are complex, and I will return to them shortly, but on one level the view of language as hegemonic yet broken, contradictory, and open to reconstruction by its speakers reinforced both the Gramscian critique and pluralist reconfiguration of American culture already underway in social-cultural historiography.

Moved by populist, socialist, and/or feminist political sympathies, social-cultural historians often valorized their subjects' resistance to oppression and sturdy survival, or conversely, their victimization by oppressors.⁵⁰ Using a symbolic anthropology that depicted culture as the primary realm of integration and meaning in peoples' lives, these historians viewed culture as the site of indigenous strength.⁵¹ This romantic tendency has been both accentuated and made more difficult with the increase in historical sophistication, egalitarian sensitivity to each historical subject, and the poststructuralist valorization not simply of differences but of difference, with its fear of essentializing any category. Because racial, ethnic, class, and gender identities crosscut each other, sometimes supporting and sometimes contradicting other identities, historians have been pushed to an increasingly atomized level of analysis. At the same time they strove to maintain the conceptual and moral integrity of social-cultural groups.⁵²

These historians nonetheless expressed the ambition to reconfigure all of American or Western history on the basis of the social-cultural history of the dispossessed. They brought with them progressive narratives of history that might effect such integration, derived from Marxism, feminist theories of patriarchy, and liberal/social democratic hybrids. While the politics of the 1960s had regenerated these progressive historical hopes, the political weakness of Marxism in the United States and the recent retreat of socialism and collapse of communism around the world have made progressive narratives difficult to sustain. At the same time, the romantic current in the social-cultural enterprise, the determination that those who have, by some standards, lost in life will not lose in historiography, makes it difficult to plot a narrative that leaves the subjects of social history undiminished. Herbert Gutman, for example, in 1981 quoted T. S. Eliot's Christian imagery of redemption, and thereby the mythic basis of both Marxist and American history, when he called for "a new synthesis . . . that incorporates and then transcends the new history" of blacks, the working class, and women.⁵³ In his own work on labor history, however, unlike his model E. P. Thompson, Gutman was unable to achieve a Marxist-like synthesis that would sustain a progressive history of the American working class.⁵⁴

Increasing disillusion with liberal politics and the liberal state has taken a heavy toll on liberal proposals for reintegration as well. Thus Thomas Bender has suggested that a synthetic history of the United States could be written around the idea of the civic sphere, developed by Jurgen Habermas and others, "as an arena for the play of cultures and interests in society and the product of that

play.” But his proposal was immediately met with criticism of the structural constraints on that civic “arena” and of the hierarchical premise of civic culture as “core” and social-cultural life as “periphery.”⁵⁵

So the progressive narratives of American and Western history promised by social theory have remained elusive. Nonetheless, the social-cultural history of the dispossessed has had a major impact on the practice of historiography in the United States, more so than that of social science history or modernization theory. The relatively decentralized university system and the conjunction of 1960s politics with the opening of academic careers to women, African-Americans, and the children of immigrants swept this new history rapidly through the discipline. Grounding their work in the accepted scholarly standards of the profession and showing that agency—and with it, politics—operates at all levels of society and culture, the social-cultural historians succeeded in multiplying the subjects of historiography and revising traditional topics.⁵⁶

If we step back for a moment to compare the new histories of the last three decades with *Annales* historiography, we can see that in many ways they have followed similar paths.⁵⁷ Like *Annales* historiography, American new histories entered into a “dialogue between history and the social sciences” that looked for the social-cultural structures and processes at work beneath the level of political events. Especially among the social science historians and modernizationists, that dialogue included the adoption of formal social scientific theories and methodologies. Most often, however, both American historians and *Annalistes* have borrowed questions, approaches, and techniques less formally. Working eclectically to suit the needs of their empirical data, they have brought multiple dimensions of analysis to bear on their studies. That loose and eclectic mode of operation often reflects historians’ superficial engagement with social theory, but it also follows from their preference for empirical richness and complexity.⁵⁸ On both sides of the Atlantic, the new histories are now less driven by their original programmatic intentions, and—as historiography has multiplied—less by theory than historiography.⁵⁹

There are also distinct differences between *Annales* historiography and the American new histories. The *Annales* emphasis on the *longue durée* has suited the French national temper far better than the American.⁶⁰ The *longue durée* focuses on the structural conditions that constrain human action, while American social thought, even in the truncated form of behaviorism, has tended to assume voluntarism. Social-cultural historians in the United States have tried to show how even ordinary people construct their own lives. The *longue durée* in France is also an alternative to progress, while American new histories have at least hoped to reestablish a sense of progress, whether the progress of a genuine historical science, or the progress inscribed in the middle-range theories of American social science, or the narratives of American and Western progress provided by Marxist and liberal social theory. It is indicative of that difference that the *Annales* program of “total” history has moved in recent years away from global ambitions to emphasize depth within a smaller compass, while many social-cultural historians in the United States have moved in the other direction, calling for larger narrative

syntheses or, in Charles Tilly's terms, analyses of "big structures, large processes, huge comparisons."⁶¹

The hallmark of post-sixties historiography in the United States, however, is the social-cultural history of dispossessed groups. And here, of course, is a major difference from *Annales* historiography. If French historians recovered the social experience and mentalité of the peasantry and *marginaux*, American historians focused specifically on women, the working class, and the diverse racial and ethnic groups that compose American society. When we move from the social-cultural history of the dispossessed to the newer cultural history that has formed in its wake, the difference is even more striking. A principal catalyst of that new cultural history is poststructural literary theory that originated in France, but given its emphasis on discontinuity and the fragmentary, it has influenced American rather than French historiography.

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The newer histories of culture, gender, race, and postcolonialism enact an alliance with the humanities rather than the social sciences, a shift in alliance linked to the declining fortunes of positivism just as historicism and theories of language were gaining new philosophical stature.⁶² The theories that have influenced these newer histories are—roughly speaking—postmodern: anti-foundationalist philosophies and poststructural literary theories that examine the linguistic construction of reality.⁶³ In the American academy, language and texts have generally come to be understood as products of history, but postmodern theory embraces a radical historicism. It pushes the qualitative difference of the past to the point of *discontinuity*, leading the historian to look for breaks and fissures that have been glossed over by previous historiography and to show how historical forms are at every moment produced and reproduced. Radical discontinuity also means radical contextualism. Not only are there no transcendental or natural kinds embedded in history that persist through time, there are no "historical individuals," no self-acting, holistic historical entities such as stages, nations, classes, intellectual disciplines, or selves. Like the "natural" categories of race and sex, these too are held to be discontinuous social-cultural constructions whose fissures and reproduction must be disclosed.⁶⁴

Postmodern theories found a welcome audience among social-cultural historians of the dispossessed who were already studying culture and already becoming self-critical about their historical categories. Through work in anthropology, particularly Geertz's textualization of culture, they were prepared for a view of culture as a system of signs.⁶⁵ The new theory encouraged them to go beyond the separate histories of groups to the social-cultural conventions or codes of gender, race, class, and ethnicity that define those groups. As a result postmodern theory has helped to catalyze an explosion of new historical energies. Women's history, already the most innovative sector of social-cultural history, was reenergized by the study of gender.⁶⁶ Gender and race became major categories of analysis, to be applied not only to women and people of color but to the white race and the male gender.⁶⁷ New topics emerged that drew on the methods and perspectives of

postmodern theory, such as the body, time, and postcolonial experience. And cultural history, including the study of popular culture, became a fast-growing frontier of historical investigation.⁶⁸

Postmodern theory also found an audience among intellectual historians. During the 1970s, intellectual historians had felt challenged by the new social history to defend their focus on elites and on ideas that seemed to float free of concrete social realities.⁶⁹ In a variety of efforts to rethink the basis of their practice, they drew on Collingwood, speech-act theory, and Kuhn's historical theory of science to argue that actions and social formations are inseparable from their meanings and that meanings are produced by speakers and writers engaged in a social enterprise. As a result of this work, intellectual historians in the United States began to think of their subject less as "ideas" than as the collective enterprise that shaped them. David Hollinger described that enterprise as the discourse of intellectuals and the term discourse was adopted by others, though it could mean a variety of things. Hollinger urged that discourses centered on questions. Others applied the term to J. G. A. Pocock's historical languages, paradigms, and traditions, and still others to looser connections of ideas and metaphors. Michel Foucault's understanding of discourse, with its emphasis on the linguistic construction of experience as an exercise of power, both reinforced and altered historians' use of the term.⁷⁰ The interest in the social construction of meanings that drew American intellectual historians toward discourse could also draw them toward the study of culture. The new cultural history was formed as both social and intellectual historians converged on the importance of culture and began to explore postmodern methods of analysis.⁷¹

Postmodern theories thus originate in different intellectual territory from the social theories we considered earlier. On one level, postmodern theories extend deeper into the historian's hermeneutic realm the structural elaboration of historicism that has been going on since early in the twentieth century. They insert into language and culture the structuralist concern with power: their interest is in how linguistic codes and systems construct subjectivity; they prefer spacial metaphors that make abstract linguistic and cultural phenomena concrete. Yet on another level, these poststructural theories move in a profoundly different direction from the global theories of progress and the analytical theories of structure and process that previously have been employed to stabilize the uncertainties of modern history. In postmodern theories, it is precisely the structural element, the constructed character of all texts and linguistic categories, that make them unstable. In this view, the social-theoretical narratives of progress and fixed theoretical categories like class and culture, by their normative inclusive character, deny their own fictionality and instability and thereby distort the creative possibilities of the present and future.⁷²

There are good reasons for the special appeal of postmodern theory in the United States. The twentieth century's brutalities and the inchoate condition of the contemporary world have stunned without entirely banishing the deep reserve of liberal faith in America. The political tendency of postmodernism is individualistic and pluralistic, if not anarchistic, motivated by fear of the mono-

lithic social order it locates in modernity.⁷³ Such a fear has deep roots in the ambivalent American individualism analyzed by Tocqueville. Since the postwar/Cold War decade, an important segment of American social thinkers and new historians have mobilized against similar threats to individual freedom, first in the form of a "soft totalitarianism" and then, on the New Left, the cultural hegemony exerted by capitalism.⁷⁴ The fear of a monolithic social order leads postmodernists toward liberal and radical versions of individualistic, pluralist politics. It also leads toward the realm of culture, a prime contemporary site of contestation between individual freedom and social constraint. Postmodern theory can thus express both the uncertainties of the present and deeply ingrained political impulses. It is no accident that in concluding her introduction to the new cultural history, Lynn Hunt asked playfully, "Are we headed here for a 'comic' ending in literary terms? An ending that promises reconciliation of all contradictions and tensions in the pluralist manner most congenial to American historians?"⁷⁵

How postmodern theory will play itself out in American historiography is an open question. Decentralization and specialization give academic disciplines in the United States enormous power to absorb and disarm disruptive innovations. Indeed, these theories have not yet penetrated very deeply into the historical profession; even those areas most strongly affected, like women's history and cultural history, are only partially shaped by them.⁷⁶ Unlike earlier social theories that provided historians with useful and reassuring tools, the tools of postmodern theory carry with them epistemological burdens. Social theory attacked the epistemological doubt that had been an undercurrent in the discipline since the 1920s, strengthening the historian's authoritative claim to discover what really happened. Postmodern theory questions that claim and urges the historian to examine doubt in her text. Social theory helped stabilize the uncertainties of twentieth-century history and of historical interpretation. Postmodern theory urges that these uncertainties be acknowledged and creatively magnified. To date, intellectual historians are more cognizant of these issues than others; many in the discipline have been scared off by a political attack on postmodern theory that caricatures its most radical implications. However, American historians who have made use of poststructural theories show no inclination to accept the reduction of all experience to textuality or of all narrative to fiction. On the contrary, most have argued that experience remains a viable category apart from textuality and that their constructed narratives produce warrantable knowledge.⁷⁷

Just as American new historians and Annalists used social theory loosely and eclectically and stopped short of the point at which historical values would be threatened, a similar outcome is likely for postmodern theories. Most historians will be more interested in reaping the empirical harvest of this new perspective than in facing or resolving its epistemological dilemmas. Nor is it likely that the focus on language and culture will eclipse all other approaches to history. Rich veins of structural analysis and social history are still productive, and social theorists themselves are exploring postmodern theories, reconfiguring what has been sharply separated as "social" and "cultural."⁷⁸ Again it is noteworthy that Lynn

Hunt has already voiced regret at the absence of social theory from new works in cultural history.⁷⁹

Postmodern theory nonetheless contributes to a new kind of alliance between history and neighboring disciplines, a process foreshadowed in the transformation of the SSHA. What is happening in the United States is not a partnership of the sort forecast by the new historians of 1912 or 1950, nor the Annales model of an integration of other perspectives within the master discipline of history. Rather it is the diffusion of historicism across disciplinary boundaries into the humanities and social sciences, producing works that are recognizably historicist yet recognizably different in approach. American historians are increasingly conscious of these historical works as well as the theory being produced in adjacent disciplines.⁸⁰ Historians' special authority may, of course, disperse along with the diffusion of their outlook.⁸¹ Yet a common, if variegated, allegiance to historicism could allow the human sciences and humanities to function more like the natural sciences, where the assurance of a common approach allows researchers to follow their problems across disciplinary lines. In either case, this new alliance between history and theory promises again to reshape the writing of history in the United States.

NOTES

I want to thank my colleagues at the San Marino conference for their stimulating comments, particularly Roger Chartier and Jacques Revel. I am also grateful for the excellent suggestions offered by Orest Ranum and Lynn Hunt and by the members of the Washington Seminar on American History and Culture, particularly James Gilbert and James Banner; and for the thoughtful comments of John R. Hall, who responded to my brief paper on this topic at the SSHA meeting in 1994.

1. This understanding of the twentieth-century new histories is derived from Georg G. Iggers, "Historicism (a Comment)," *Historia Historiographie* 10 (1986): 131–44, and Ignacio Olabarri, "'New' New History: A *Longue Durée* Structure," *History and Theory* 34 (no. 1, 1995): 1–29. Following an earlier suggestion of Iggers in *New Directions in European Historiography*, Ian Tyrrell has developed the comparison between the first New History in the United States and Annales historiography in *The Absent Marx: Class Analysis and Liberal History in Twentieth-Century America* (New York, 1986), chap. 2. See also Ernst A. Breisach, "Two New Histories: An Exploratory Comparison," in *At the Nexus of Philosophy and History*, ed. Bernard P. Dauenhauer (Athens, Ga., 1987). The quotation is from Iggers, "Historicism," 140.

2. There are a number of definitions of historicism. Karl Popper identified historicism as a hybrid of naturalistic conceptions of law and historicist conceptions of change in *The Poverty of Historicism* (Boston, 1957). Maurice Mandelbaum limited historicism to teleological doctrines of historical development and specifically excluded the historian's "historical sense," in *History, Man, and Reason: A Study of Nineteenth Century Thought* (Baltimore, 1971). My view of historicism follows one usage common in intellectual history and draws from Iggers, "Historicism," 131–40; Friedrich Meinecke, *Historism*, trans. J. E. Anderson (London, 1972); Hayden V. White, "On History and Historicisms," introduction to Carlo Antoni, *From History to Sociology: The Transition in German Historical Thinking* (Detroit, 1959), xv–xxviii; and J. G. A. Pocock, *Politics, Language and Time* (New York, 1973).

3. This point is implied in Iggers, "Historicism," 141–42, and developed in Olabarri, "New' New History," 4–11.

4. Dorothy Ross, *The Origins of American Social Science* (Cambridge, 1991), chap. 1.

5. In calling the view of American history that Bancroft exemplified "exceptionalism," I resist the argument of the admirable essay by Daniel Rodgers in chapter 1 of this volume. To limit the term to Turner and post-1945 histories misses the way in which Bancroft, as well as many Teutonist historians, understood American history as an exception from salient, universal processes of history. Precisely because America gathered in the historical seeds of liberty from around the world and enacted the ideal toward which universal history moved, it was exempt from the destructive historical forces that shaped the histories of all other countries. I believe it is more than homology that links the millennial identification of the American republic with Turner's seating of the universal frontier process in the United States, and Bancroft's Hegelianism with the American historical dynamics inspired by Marx. For this more expansive view of American exceptionalism, see Ross, *Origins*, pt. 1; Dorothy Ross, "Historical Consciousness in Nineteenth-Century America," *American Historical Review* (AHR) 89 (October 1989): 909–28, and "Grand Narrative in American Historical Writing: From Romance to Uncertainty," AHR 100 (June 1995): 651–77. On Motley and Prescott as well as Bancroft, see David Levin, *History as Romantic Art* (Stanford, 1959).

6. This view of historico-politics as the context for the founding of the historical discipline in the United States and seedbed for the New History is set out in Ross, *Origins*, chaps. 3 and 8.

7. James Harvey Robinson, *The New History: Essays Illustrating the Modern Historical Outlook* (New York, 1912), 14.

8. Although he does not link it to a revision of American exceptionalism under the deeper influence of historicism, Leonard Krieger notes this shift in American historians' treatment of European history, in "European History in America," in John Higham, Krieger, and Felix Gilbert, *History* (Englewood Cliffs, N.J., 1965), 241, 263, and chaps. 2–3, *passim*.

9. Ross, *Origins*, 270–74.

10. Dorothy Ross, "The 'New History' and the 'New Psychology': An Early Attempt at Psychohistory," in *The Hofstadter Aegis*, ed. Stanley Elkins and Eric McKittrick (New York, 1974), 207–34. In Robinson, *New History*, see particularly chaps. 1 and 3.

11. For illuminating discussions of Beard and the New History, see Tyrrell, *The Absent Marx*, chaps. 1–2, and Ernst A. Breisach, *American Progressive History* (Chicago, 1993). On Beard's influence, see also Terrence J. McDonald, "Theory and Practice in the 'New' History: Rereading Arthur Meier Schlesinger's *The Rise of the City, 1878–1898*," *Reviews in American History* 20 (1992): 432–45. McDonald shows that Arthur M. Schlesinger used Beard and Seligman's economic interpretation in his urban history, but at such a low level of specificity that it was taken for no theory at all. As McDonald points out, Beard's reputation as a radical also probably helped to blur Schlesinger's analysis.

12. Ross, *Origins*, chap. 9 and 442–44.

13. *Ibid.*, chap. 10.

14. The key text in the structural transformation of postwar/Cold War American social science is Talcott Parsons, *The Structure of Social Action* (New York, 1937). For surveys of the structural movement in a number of social sciences, see Fred Matthews, "Social Scientists and the Culture Concept, 1930–1950: The Conflict between Processual and Structural Approaches," *Sociological Theory* 7 (1989): 87–101, and Dorothy Ross, "Social Science," in *A Companion to American Thought*, ed. Richard Wightman Fox and James T. Kloppenberg (Oxford, 1995), 634–37.

15. Edward A. Purcell, Jr., *The Crisis of Democratic Theory: Scientific Naturalism and the Problem of Value* (Lexington, Ky., 1973), pt. 4. For liberals, this shift in historical consciousness was prepared by the depression and theories of a mature economy; for the left, by the stalemate between the discredited alternatives of Marxism and liberalism. See Howard Brick, *Daniel Bell and the Decline of Intellectual Radicalism: Social Theory and Political Reconciliation in the 1940s* (Madison, 1986).

16. This theme is implicit and explicit in many texts of this period. Among historians, see Louis Hartz, *The Liberal Tradition in America* (New York, 1955), chap. 11; C. Vann Woodward, "The Irony of Southern History" (1953), in *The Burden of Southern History* (New York, 1960).

17. Richard Hofstadter, "History and the Social Sciences," in *Varieties of History*, ed. Fritz Stern (New York, 1956); Richard Hofstadter, "History and Sociology in the United States," in *Sociology and History: Methods*, ed. Hofstadter and Seymour Martin Lipset (New York, 1968); H. Stuart Hughes, "The Historian and the Social Scientist," *AHR* 60 (1960): 20–46; David M. Potter, *People of Plenty: Economic Abundance and the American Character* (Chicago, 1954), introduction; Edward N. Saveth, ed., *American History and the Social Sciences* (Glencoe, 1964).

18. On the response to Beard's and Becker's relativism, see Tyrrell, *The Absent Marx*, 22–23, 36–39, 93–95, although, unlike Tyrrell, I believe this uncertainty was only partially contained and never vanquished; on Becker, Carl L. Becker, *The Heavenly City of the Eighteenth-Century Philosophers* (New Haven, 1932); Burleigh Taylor Wilkins, *Carl Becker: A Biographical Study in American Intellectual History* (Cambridge, 1961).

19. Hofstadter, "History and the Social Sciences," 370; Potter, *People of Plenty*, xii. For Hughes, the social sciences also offered to make generalization more precise, as well as to make historians' weakly conceptualized fields of economic, social, and cultural history more coherent. Hughes, "The Historian and the Social Scientist."

20. Richard Hofstadter, "History and Sociology in the United States," in *Sociology and History*, 18.

21. For a view of postwar/Cold War historiography as more complacent and monolithic than I suggest here, see Peter Novick, *That Noble Dream: The 'Objectivity Question' and the American Historical Profession* (Cambridge, 1988), pt. 3.

22. For example, Richard Hofstadter's *American Political Tradition* (New York, 1948). See Hofstadter, "The Importance of Comity in American History," *Columbia University Forum*, 13 (winter 1970): 9.

23. This connection is drawn by Wilfred M. McClay in *The Masterless: Self and Society in Modern America* (Chapel Hill, 1994), chaps. 6–7, a superb study of the symbiotic relationship between autonomous individualism and social absorption in the nineteenth and twentieth centuries.

24. Saveth, *American History and the Social Sciences*, 17–18. On theories of the "middle range," see Robert Merton, *Social Theory and Social Structure*, rev. ed. (New York, 1963).

25. Examples are Richard Hofstadter, *The Age of Reform* (New York, 1955) and *The Paranoid Style in American Politics* (New York, 1965); David Donald, "Toward a Reconsideration of Abolitionists," in *Lincoln Reconsidered* (New York, 1956); Marvin Meyers, *The Jacksonian Persuasion* (New York, 1960). William H. Sewell, Jr., remarks on this cast of 1950s social science history in "Introduction: Narratives and Social Identities," *Social Science History* (SSH) 16 (fall 1992): 487.

26. Krieger, "European History in America," chaps. 4–5. Examples are Franklin L. Ford, *Robe and Sword: The Regrouping of the French Aristocracy after Louis XIV* (Cambridge, 1953), and H. Stuart Hughes, *Consciousness and Society: The Reconstruction of European Social Thought 1890–1930* (New York, 1961 [1958]).

27. For an early sampling, see Robert P. Swierenga, ed., *Quantification in American History: Theory and Research* (New York, 1970).

28. Novick, *That Noble Dream*, chap. 13.

29. Lynn Hunt, ed., *The New Cultural History* (Berkeley, 1989), introduction and pt. 1.

30. Novick, *That Noble Dream*, 362–67, 470.

31. See the selections in Swierenga, ed., *Quantification in American History*, and Lee Benson, *Toward the Scientific Study of History* (Philadelphia, 1972).

32. My account of the social science history that formed around the SSHA draws largely on Andrew Abbott, “History and Sociology: The Lost Synthesis,” in *Engaging the Past: The Uses of History across the Social Sciences*, ed. Eric H. Monkkonen (Durham, 1994), 77–112, an excellent account of the movement. On this model of American social science, see Christopher G. A. Bryant, *Positivism in Social Theory and Research* (New York, 1985), chap. 5.

33. The information on relative participation comes from Donna Gabaccia, “Interdisciplinary Communication and SSHA Annual Conferences: A Program Chair’s Perspective,” *SSHA News* (winter 1995): 8–10.

34. For an excellent account of the Annales method of “serial history,” see Robert Forster, “Achievements of the Annales School,” *Journal of Economic History* 38 (March 1978): 58–76. Ronald P. Formisano, “The Invention of the Ethnocultural Interpretation,” *AHR* 99 (April 1994): 453–77, is a revealing history and defense of ethnocultural voting studies; the best critique is still Richard L. McCormick, “Ethno-cultural Interpretations of Nineteenth-Century American Voting Behavior,” *Political Science Quarterly* 89 (June 1974).

35. One obstacle faced by social science history—the time-consuming difficulty of gathering data—speaks to the difference between the individualistic and decentralized historical discipline in the United States and the centralized historical program of the Annales. See Forster, “Achievements of the Annales School.”

36. Gabaccia, “Interdisciplinary Communication,” 9.

37. As Edward Berkowitz tells me, political historians who feel marginalized in an historical profession predominantly devoted to social and cultural history continue to find the SSHA a welcome venue for meeting the historically oriented political scientists who share their interests.

38. Sewell, “Introduction: Narratives and Social Identities.”

39. Thus Abbott regards the SSHA alliance as a mismatched failure, in “History and Sociology”; for an historian’s disappointment, see Allan G. Bogue, “Great Expectations and Secular Depreciation: The First Ten Years of the Social Science History Association,” *SSH* 11 (fall 1987): 329–42, and Eric H. Monkkonen, “Lessons of Social Science History,” *SSH* 18 (summer 1994): 161–68.

40. The best introduction is the long entry on “modernization” written by two architects of the theory, Daniel Lerner and James S. Coleman, in *International Encyclopedia of the Social Sciences*, 10:386–402.

41. Peter Burke, *History and Social Theory* (Ithaca, 1992), 132–41.

42. Robert Wiebe, *The Search for Order* (New York, 1967). Wiebe reported in a talk at the American Historical Association, December 3, 1996, that he had known the work of Max Weber and Robert Merton but not the work of 1950s theorists of “third world” modernization. See also Kenneth Cmiel, “Destiny and Amnesia: The Vision of Modernity in Robert Wiebe’s *The Search for Order*,” *Reviews in American History* 21 (1993): 352–68.

43. Wiebe, *Search for Order*, 300.

44. Robert Wiebe, *The Segmented Society: An Historical Preface to the Meaning of America* (New York, 1975). The subtitle is revealing. Wiebe argued for “the persisting segmentation of American society” and relocated the source of harmony to a “unique pattern of relationships” among those segments, which constitute a “consistent, interdependent system”

based on "widespread agreement on certain fundamentals" (x-xi, 13). Alan Brinkley noted that Wiebe had retreated to a position of incomplete modernization, in "The Problem of American Conservatism," *AHR* 99 (April 1994): 427.

45. Louis Galambos, "The Emerging Organizational Synthesis in Modern American History," *Business History Review* 44 (1970): 279-90, and "Technology, Political Economy, and Professionalization: Central Themes of the Organizational Synthesis," *ibid.*, 57 (1983): 471-93.

46. Harry Liebersohn, *Fate and Utopia in German Sociology, 1870-1923* (Cambridge, 1988).

47. See Thomas Bender, *Community and Social Change in America* (Baltimore, 1982 [1978]).

48. For a sympathetic review of some of this literature, see Gordon S. Wood, "Inventing American Capitalism," *The New York Review of Books* (June 9, 1994), 44-49.

49. Henry Abelove et al., eds., *Visions of History* (New York, 1983), particularly the interviews with Linda Gordon, Natalie Zemon Davis, and Herbert Gutman; Warren I. Susman, *Culture as History: The Transformation of American Society in the Twentieth Century* (New York, 1984), introduction and headnotes.

50. Michael Kammen, ed., *The Past before Us: Contemporary Historical Writing in the United States* (Ithaca, 1980), particularly Peter N. Stearns, "Toward a Wider Vision: Trends in Social History."

51. Lawrence W. Levine, "Clio, Canons, and Culture," *Journal of American History* (JAH) 93 (1993): 864; Suzanne Desan, "Crowds, Community, and Ritual in the Work of E. P. Thompson and Natalie Davis," in Hunt, ed., *New Cultural History*, 47-71; Ira Berlin, "Introduction: Herbert G. Gutman and the American Working Class," in Gutman, *Power and Culture: Essays on the American Working Class*, ed. Berlin (New York, 1987), 36-45; Carroll Smith-Rosenberg, "The Female World of Love and Ritual: Relations between Women in Nineteenth Century America," *Signs* 1 (1975): 1-29.

52. See, for example, David Roediger, "Race and the Working-Class Past in the United States: Multiple Identities and the Future of Labor History," *International Review of Social History* 38 (supplement, 1993): 127-43.

53. Herbert G. Gutman, "The Missing Synthesis: What Ever Happened to History?" *Nation* (November 21, 1981), 554. For other calls for a new synthetic history based in working-class history, gender, and race, see J. Carroll Moody and Alice Kessler-Harris, eds., *Perspectives on American Labor History: The Problems of Synthesis* (DeKalb, Ill., 1990), and Roediger, "Race and the Working-Class Past."

54. On the comparison of Gutman and Thompson, see also Tyrrell, *The Absent Marx*, 146-54.

55. Thomas Bender, "Wholes and Parts: The Need for Synthesis in American History," *JAH* 73 (June 1986): 120-36, and "A Round Table: Synthesis in American History," *JAH* 74 (June 1987): 107-30.

56. The massive publication of the Freedmen's Bureau papers, edited and interpreted by a team under Ira Berlin, for example, has both enlarged and shifted the focus of Reconstruction historiography: *Freedom: A Documentary History of Emancipation, 1861-1867* (Cambridge, 1982-); see also Eric Foner, *Reconstruction: America's Unfinished Revolution, 1863-1877* (New York, 1988). The ways in which women's civic participation reconfigured politics and shaped the welfare state is now a major topic in the study of the United States and of Europe: Mary Ryan, *Women in Public: Between Ballots and Banners, 1825-1880* (Baltimore, 1990); Paula Baker, *The Moral Frameworks of Public Life: Gender, Politics, and the State in Rural New York, 1820-1930* (New York, 1991); Linda Gordon, *Pitied but Not En-*

titled: *Single Mothers and the History of Welfare* (New York, 1994); Seth Koven and Sonya Michel, eds., *Mothers of a New World: Maternalist Politics and the Origins of Welfare States* (London, 1990).

57. My comparisons here are drawn against Forster, "Achievements of the Annales School."

58. For an excellent analysis of historians' failures in this regard, see Terrence J. McDonald, "Faiths of Our Fathers: Middle Range Social Theory and the Remaking of American Urban History, 1940–1985," in *American City History: Modes of Inquiry*, ed. Kathleen Neils Conzen, Michael H. Ebner, and Russell Lewis (Chicago, 1992).

59. Galambos makes this point for the organizational synthesis, but it is also true of the other new histories. Galambos, "Technology, Political Economy, and Professionalization," 471, 493.

60. As Gordon Wood points out in chapter 7 of this volume, only the colonial period of American history provided American historians with something like a *longue durée* of a century or more.

61. Charles Tilly, *Big Structures, Large Processes, Huge Comparisons* (New York, 1985). On the reformulation of total history, see Olabarri, "New New History," 17–18. On differences between American New History and the Annales school, Breisach, "Two New Histories," finds interesting lapses in Annales historians' rejection of progress.

62. A good way to track this shift is through the work of Richard J. Bernstein: *The Restructuring of Social and Political Theory* (New York, 1976); *Beyond Objectivism and Relativism: Science, Hermeneutics, and Praxis* (Philadelphia, 1985), and *The New Constellation: The Ethical-Political Horizons of Modernity/Postmodernity* (Cambridge, 1992).

63. Introductions to this body of theory that are especially useful in this context are Allan Megill, *Prophets of Extremity: Nietzsche, Heidegger, Foucault, Derrida* (Berkeley, 1985); Bernstein, *The New Constellation*; Lynn Hunt, "Introduction: History, Culture, and Text," in Hunt, ed., *New Cultural History*; Terry Eagleton, *Literary Theory* (Minneapolis, 1983), chap. 4.

64. The historicist direction of American literary theory is exemplified in Frank Lentricchia, *After the New Criticism* (Chicago, 1980). See also Brook Thomas, *The New Historicism and Other Old-Fashioned Topics* (Princeton, 1991). On radical historicism, see Michel Foucault, "Nietzsche, Genealogy, History," in *Language, Counter-Memory, Practice*, ed. Donald Bouchard (Ithaca, 1988).

65. Hunt, "Introduction: History, Culture, and Text," and Aletta Biersack, "Local Knowledge, Local History: Geertz and Beyond," in Hunt, ed., *New Cultural History*.

66. Historians of women, for example, quickly found it useful to discuss the way culture "constructs" gender differences and the way women refigure those differences in their own lives. See, for example, Joan Scott, *Gender and the Politics of History* (New York, 1988).

67. David Roediger, for example, has convincingly shown that male gender and white race-consciousness were crucial to the forging of the antebellum working class in the United States. David Roediger, *The Wages of Whiteness: Race and the Making of the American Working Class* (London, 1991).

68. Hunt, ed., *New Cultural History*.

69. See John Higham and Paul Conkin, eds., *New Directions in Intellectual History* (Baltimore, 1979).

70. This history is reviewed in David A. Hollinger, "Running To and Fro: Discourse, Historians, and Daniel's Dream," paper delivered at the San Marino conference, June 1995. See also Hollinger, "Historians and the Discourse of Intellectuals," in Higham and Conkin, eds., *New Directions*, 42–63; J. G. A. Pocock, "Introduction: The State of the Art," in *Virtue*,

Commerce, and History (Cambridge, 1985); Jan Goldstein, "Foucault among the Sociologists: The 'Disciplines' and the History of the Professions," *History and Theory* 23 (1984): 170–92.

71. That joint convergence is visible in Hunt, ed., *New Cultural History*, and Richard Wightman Fox and T. J. Jackson Lears, eds., *The Power of Culture: Critical Essays in American History* (Chicago, 1993).

72. Terry Eagleton, *Literary Theory* (Minneapolis, 1983), chap. 4.

73. On the centrality of this fear in postmodern theory, see John McGowan, *Postmodernism and Its Critics* (Ithaca, 1991).

74. McClay, *The Masterless*. See note 23 above.

75. Hunt, ed., *New Cultural History*, 22.

76. The distinction often made between cultural history and cultural studies, for example, marks a line most historians are reluctant to cross into a fuller embrace of postmodern theory.

77. See, for example, John E. Toews, "Intellectual History after the Linguistic Turn: The Autonomy of Meaning and the Irreducibility of Experience," *AHR*, 92 (1987): 879–907; Gabrielle M. Spiegel, "History, Historicism, and the Social Logic of the Text in the Middle Ages," *Speculum* 65 (1990): 59–86; Nancy Partner, "History without Empiricism/Truth without Facts," in *Transformations: The Languages of Culture and Personhood after Theory*, ed. Christie McDonald and Gary Wihl (State College, Pa., 1994), 1–10. This can be said even of Hayden White and Dominick LaCapra, who go furthest in acceptance of the theoretical claims of postmodernism but continue to produce historical narratives that follow the conventional rules of the profession's realist historiography. See LaCapra, *Rethinking Intellectual History: Texts, Contexts, Language* (Ithaca, 1983).

78. The works of sociologists Pierre Bourdieu and Richard Harvey Brown and of the social theorist Richard J. Bernstein point in this direction.

79. Lynn Hunt, "History Beyond Social Theory," in *The States of 'Theory': History, Art, and Critical Discourse*, ed. David Carroll (New York, 1993).

80. One sign is the books being reviewed in the leading reviewing journal in American history. In a recent number singled out by Morton Keller as indicative of the new orientation of American historiography, at least ten of the forty-three books reviewed were by scholars in disciplines other than history. *Reviews in American History* 21 (December 1993); Morton Keller, "Reviews in American History," *Times Literary Supplement* (March 18, 1994), 22.

81. For example, in a recent survey of the historical work being done in the social sciences, the authors found that historical social scientists looked to each other rather than to historians for their authorities, creating subfields within rather than across disciplinary borders. Monkkonen, ed., introduction to *Engaging the Past*.

Explaining Racism in American History

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IN 1940, the African-American intellectual and political activist W. E. B. Du Bois described in *Dusk of Dawn* his intellectual odyssey of fifty-odd years. He noted that upon his graduation from Harvard in the 1890s, he had thought of “[t]he Negro problem [as] a matter of systematic investigation and intelligent understanding. The world was thinking wrong about race, because it did not know. The ultimate evil was stupidity. The cure for it was knowledge based on scientific investigation.” Consequently, he decided to address the problem by studying “the facts, any and all facts, . . . and by measurement and comparison and research, work up to any valid generalization which I could.” But soon “there cut across this plan which I had as a scientist, a red ray which could not be ignored. . . . a poor Negro in central Georgia, Sam Hose, had killed his landlord’s wife. I wrote out a careful and reasoned statement concerning the evident facts and started down to the Atlanta *Constitution* office, . . . I did not get there. On the way news met me: Sam Hose had been lynched, and they said that his knuckles were on exhibition at a grocery store farther down on Mitchell Street, along which I was walking. . . . I turned back to the University. I began to turn aside from my work.”¹

Much like Du Bois, we—scholars and laypersons alike—are still frustrated by our continuing incapacity to explain racism or most racial phenomena. Indeed, our intellectual problems are not unlike those Du Bois confronted almost a century ago: Is racism a phenomenon best understood as caused by misinformation and ignorance or by deep irrational urges and psychological dysfunction? Or more precisely, perhaps: we have made some progress in explaining the origins of racism, but not its reproduction. There is a growing consensus that the origins of racism are linked with the advent of modernity; that is, to developments in the aftermath of European overseas expansion and colonization in the fifteenth and sixteenth centuries.² Thus we have come to understand racism not as something “transhistorical” or “epiphenomenal,” something outside normal historical and social processes, but as social creations under determinate historical circumstances.³ But even as the concepts of race and racism have become thus historicized, our notions of racism—in historical literature as well as in lay thought—remain in many other respects stubbornly naturalized. Even some of the best historical literature and some of the more prominent historical treatments of America’s racial history written since 1960 illustrate the intellectual difficulties we confront. That is to say, very often they offer insightful, sometimes subtle,

and potentially illuminating examinations of the development of racial ideas, of the “historical” determination of racial phenomena, and even of the “social construction” of racial concepts, only to regress to forms of explanation that are at their core often functionalist, occasionally reductionist, and sometimes even biologicistic.

Indeed, explaining racist phenomena confronts contemporary social theory with some of its most profound challenges, intellectual and political. It puts in play some of the crucial issues we confront in attempting to elaborate a social theory adequate to contemporary society. It illuminates especially our difficulty in reconciling materialist with symbolic/discursive approaches to explaining social phenomena, and of determining the locus and nature of their interaction or fit. It exposes a fundamental discontinuity between most behavioral explanations sited at the individual level of human experience and those at the level of society and social forces.

Obviously these intellectual problems are neither exclusively American nor distinctively the province of historians. Indeed, many of our key insights into strategies of explanation for racial phenomena come from social scientists other than historians, or historians other than Americans.⁴ Nonetheless, national historiographies of racism are in some respects quite divergent. British, French, German, and Brazilian discussions of race, for example, have developed very differently, with different objects of study, prompted by different political and social concerns, and informed perhaps by different historical and intellectual trajectories. A thorough comparative examination of these distinctive national discussions is beyond the capacity of this—and perhaps any—brief paper to undertake, but even cursory, selective observations on some of the differences and similarities might be suggestive of the distinctive intellectual and institutional terrain on which the American historiography has flourished.

Formal discussion of the problem of race in America, dating at least since Thomas Jefferson's *Notes on the State of Virginia* (1785), has from the start carried a certain concreteness, wherein the social history of settlement, nation building, and public policy were explicitly and inextricably linked to issues of racial identifications and distinctions *within its own national territory*. This has given a kind of prominence, even urgency to American discussions of race throughout its history that one finds among European scholars largely in the post–World War II era.

Consequently, perhaps, race in America has acquired heretofore—for Europeans no less than for Americans—a kind of “exceptionalist” character; exceptionally rigid, pervasive, and violent. As with most “exceptionalist” assumptions, the notion that American racism was somehow unique rested on a very selective parsing of the historical record. Although American racism was profoundly shaped by the existence of slavery on its own soil, for example, European societies were also thoroughly implicated in colonial slave regimes that strained their laws, their politics, and their social mores. As decolonized subjects have sought refuge in their respective metropolises in the post–World War II era, those repressed historical issues have also emerged—and with a vengeance. In America, the land of immigrants, race relations have been indelibly marked by succes-

sive waves of white and nonwhite immigration, with each wave redefining the meaning of “race” or provoking new episodes of “racial” tension or both. But similar phenomena—of guest workers and ex-colonials—have stimulated re-examinations of race and immigration among European scholars in the late twentieth century that raise analytic problems comparable to the older American discussions.⁵ In the *longue-durée*, therefore, European and American race relations may well reveal more convergence than divergence; America may be less the unique exemplary of the profound analytic difficulties Du Bois sketched almost a century ago than simply the earliest.

It is true, nonetheless, that the historiographical trajectory of the American discourse on race has been shaped by its distinctive history. Slavery, the sectional controversy it gave rise to, and the Civil War that destroyed it have defined decisively America’s history and American historical studies. Despite their best efforts historians could never completely segregate race from these fundamental issues of national integrity. The issues raised by African-Americans were present, even when they themselves were invisible. Arguably this was less true for other victims of American racism, such as Native Americans, Mexican-Americans, and Asian-Americans, notwithstanding the clear importance of their respective histories in the formation of the American nation. For this reason, perhaps, the African-American experience has formed the template—rightly or wrongly—for studies of all other racializing experiences in America. For much the same reason, successive African-American movements for civil rights and cultural revival in the 1960s and 1970s laid the basis for the reexamination of race and racism in American historical scholarship. Since the 1960s, studies of slavery have focused much less on the traditional issues of sectional politics and southern nationalism than on the institutional and experiential qualities of slavery itself. Studies of emancipation and its aftermath now focus much less on arcane struggles among national legislative factions than on the evolution of repressive social and economic systems. As a result of these trends, the African-American experience—especially their racial victimization—has emerged at center stage in the larger national historical experience.

Recapturing the experience of racialized groups is not synonymous, however, with an explicit examination of racism as such. Thus the *explanation* of racial phenomena remains largely implicit in much of the historical literature on African-Americans, Native Americans, Mexican-Americans, and Asian-Americans. Rather than attempt a broad survey of the American historiography as such, therefore, I will focus on some of the major texts that have addressed the problematic of racism explicitly and that have deployed exemplary strategies of explanation. For our purposes the major approaches or paradigms for explaining racism can be effectively summarized under four rough headings: (1) an idealist paradigm, that is, the notion that racism is a consequence of racist ideas, a product of thought; (2) an economic or materialist paradigm, the notion that racism is a function of economic exploitation or competition; (3) a psychological paradigm, that racism arises from pathologies in the unconscious and/or conscious minds of individuals; and (4) a cultural paradigm, that racism is an aspect of

specific cultural formations in a given society. Since these approaches are not mutually exclusive, any given explanation may rely on combinations of more than one or even all of them. But their separation allows for a clearer picture of how the “triggering mechanism” of race is envisioned in each.

The Idealist Paradigm. The simplicity of the syllogism that racist ideas lead to racist actions is powerful and ubiquitous. It has provided the most common analytic framework for both popular and academic analyses of racism. As Du Bois explained, before the Sam Hose incident he, too, had been convinced that racism was simply a matter of ignorance and its solution required only new and better information and right thinking. In short, people had the wrong ideas about race, and they could simply be educated out of those ideas. The problem is that the exact causal relationship between ideas and behavior is not nearly as uncomplicated as this argument assumes. Ideas are not autonomous but mediated by social structures and processes.

One of the more comprehensive studies of this genre is Reginald Horsman's *Race and Manifest Destiny*.⁶ Horsman traces the evolution of racist ideas from European originators of Aryan and Anglo-Saxon myths of racial superiority to their descendants in America. He shows rather convincingly how various intellectual and political leaders took up or rediscovered elements of the old myths and rearticulated them in the new American context. The ideas of a great westward march of progress out of India and across the seas, of the need for racial purity, and so forth all found utility and resonance as justifications for enslaving Africans, exterminating Native Americans, and taking the land of Mexicans.

But there is an almost studied ambiguity in Horsman's discussion of historical causality. He seems on the mark when describing the very plasticity of European intellectual traditions that permitted “re-inventions” of those traditions in the Americas. The Irishman, for example, “a lazy, ragged, dirty Celt when he landed in New York,” would become “the vanguard of the energetic Anglo-Saxon people” once he reached California.⁷ But at other moments it is not entirely clear just what the explanatory status of “ideas” is. They appear at times to be nearly autonomous entities, nourished by—but not created by—historically specific social contexts. They are utilitarian, integral, and self-contained; they “fell,” they were “used,” they could “assuage.”

The new [racial] ideas *fell* on fertile ground in the 1830s and 1840s. In a time of rapid growth and change, with its accompanying insecurities and dislocations, many Americans found comfort in the strength and status of distinguished racial heritage. The new racial ideology could be *used to force* new immigrants to conform to the prevailing political, economic, and social system, and it could also be *used to justify* the sufferings or deaths of blacks, Indians, or Mexicans. Feelings of guilt could be *assuaged* by assumptions of historical and scientific inevitability.⁸

At times they are plastic ideological instruments consciously deployed to achieve certain political and material objectives—like Indian removal or the conquest of Mexico. And yet again, they sometimes appear to arise out of individual

psychological needs, to be projections of and deflections from guilty consciences pursuing a not-so-manifest destiny.

But even as Horsman demonstrates how preexisting racist ideas rationalized racial oppression and inequality in nineteenth-century America, his evidence also reveals how new ideas or new twists on old ideas developed out of particular ideological conjunctures and historical confrontations—in other words, in some instances the inequality and oppression came first. For example, Indians were seen as *potentially* white by Thomas Jefferson; they represented not savagery but innocence, an enduring emblem of nature and the wilderness Euro-Americans wanted to tame. Thus, in marked contrast with his ideas about Africans, Jefferson could urge a government policy designed to encourage Indians to be civilized and assimilated. With the growth of the cotton trade, however, other southerners came increasingly to see Indians as barriers to economic progress because they occupied some of the best cotton lands of the South. So under Andrew Jackson they were forcibly removed from these lands, many of them—like the Seminoles in Florida—with great violence and brutality. Concurrent with this expulsion there developed a strikingly different view of Indians: not natural but savage, not candidates for eventual assimilation but for extermination.⁹

Ideas, then, are not autonomous from material and political realities. In some instances they shape our behavior; in others they are altered in response to what we do. In still other cases they appear to have no relation at all to what we do. They are certainly relevant to any explanation of racism, but seem not *in themselves* to be adequate explanations for racist phenomena.

The Economistic Paradigm. At first blush, the brute realities of economic interests appear to offer a more reliable explanation of racist behavior. These are what Du Bois, referring to African-Americans, called the “rational, conscious determination of white folk to oppress us.” Simply stated, it is in the clear economic interest of some people to oppress or discriminate against other people, and such economic oppression lies at the root of racial oppression more generally.

There are two basic forms of this argument, however: one blames the ruling classes as cynical exploiters; another blames the white working class as vicious reactionaries to black competition. In the first case, the argument goes, it is in the interest of a ruling class to exploit a “racialized” workforce because the workers are less able to fight back, either because racism rationalizes or justifies their exploitation (slavery for example), or because they are divided from and against other workers and therefore become easier to control. In the second case white workers become the main oppressors because in a racially divided society the racial Other offers direct or potential competition for jobs and other economic resources and a vulnerable target for attack.

There is little doubt that there are specific historical and contemporary situations in which these explanations are especially powerful in explaining racist tensions and outbursts, but generally they offer unsatisfactory or incomplete accounts of either the original process of differentiation or its reproduction over time. Alexander Saxton’s examination of white workers’ reaction to Chinese labor

in California is a case in point.¹⁰ The role of wealthy capitalists in the creation and manipulation of a racially segmented labor force is convincingly demonstrated, as are the lines of ethnic cleavage and hostility among the working class. But confronting the questions of why Asians were singled out as the “racial” Other (rather than Irishmen or other “benighted” Europeans as found at various moments in the Northeast), and how the system reproduced itself once the original cause was no longer relevant, Saxton turns to political and ideological domains. As he notes at one point in concluding the narrative: “Entanglement of an economic conflict over contract labor with older ideological and organizational cleavages precluded any single or simple solution.”¹¹

Examining economic forces and structural contexts is essential to understanding how a racialized social order is constituted; how, for example, those Euro-American and Asian-American workers were brought into an arena of conflict in the first place. But once the precipitating economic cause or friction is removed, why does racism continue to be reproduced in the society? Once Africans or Asians or Mexicans cease to be an important reservoir of labor—as slaves, sharecroppers, or cheap industrial workers—why does racial hostility continue and in fact often increase? These questions cannot be answered by analyses that simply *reduce* race to class. For most of the nineteenth century certainly the competition blacks posed to “white” jobs was not proportionate to the violent responses of white workers—at least not in the Northeast, where African-American workers were few in number and largely excluded from growth industries.¹² And clearly, all groups who are economically exploited or economic competitors are not thought of or treated the same. As Du Bois observed bitterly in the early 1930s, whites as well as blacks were attacked for being scabs and strikebreakers, but the white scabs were attacked to scare them off or recruit them into the unions; the black scabs were attacked to kill them.¹³

Edmund S. Morgan’s explanation of the development of slavery and racism in colonial Virginia represents a more subtle model for integrating materialist and ideological analyses to explain racist phenomena. In addressing that perennial chestnut of which came first, racism or slavery, Morgan argues convincingly that whatever the racial attitudes or prejudices of white planters in seventeenth-century Virginia, their actual treatment of African and white labor was not nearly so differentiated as it would become in the late colonial period. The transformation in their attitudes and treatment he traces to material and demographic changes in the colony. Africans were reduced to slavery when the life expectancy of workers increased to a point where it was *profitable* to own a slave for life as opposed to an indentured servant for a term of years; *and* when the political and military pressure exerted on the colony by a growing (longer-living) sector of landless white ex-indentured servants made it safer to subjugate Africans to slavery than to hire more white laborers.¹⁴

But although Morgan successfully explains the differential treatment of white and black labor and the origins of slavery and racism, an argument of this form will not explain why and how the system reproduced itself throughout another century of slavery, through another half century of sharecropping, through wage-

labor in the twentieth century, down to the late twentieth century when black labor has become increasingly redundant. Again, no effective explanation can exclude the linkage between racial thought and practice and the changes in material and economic life, but that alone is inadequate to a full explanation of racism's development or its maturity. Again, after conceding the reasons for the original sin—the actions that marked and institutionalized the difference between white and black—how do we explain Sam Hose's broken and dismembered body?

The Psychological Paradigm. In his dissatisfaction with the inadequacy of both idealist and materialist explanations for “the red ray” that crossed his path, Du Bois turned to what he called “age-long complexes sunk now largely to unconscious habit and irrational urges.” The unconscious and the “irrational” fall within the province of psychological explanations, and most of the most prominent historical works drawing on this approach to explain racism have emphasized some variant of Freudian analysis.¹⁵ In its general form, as found in historical studies, the psychological argument is that personal anxieties produced in individual human beings are evaded by projecting them onto an object of aggression, an outsider. Thus the common causal triad runs: “repression,” “projection,” “oppression.”

One influential example of the direct application of such a psychological analysis to the history of racism in America is the work of Joel Kovel. Although not a historian, Kovel makes explicit much of the analytical apparatus many historians employ. He argues, among other things, that the “natural” association of blackness with dirt and excrement together with sexual anxieties stemming from our childhood development—if unresolved—will be projected onto black people as objects of fantasy and invention, and that this helps explain racist impulses.¹⁶

Although Kovel goes on to attempt to relate these purely individual traumas to the larger social and cultural orders, the latter appear to be mere metaphorical extensions to the social level of individual-level, psychological traits. As such, Kovel's explanation exposes one of the biggest hurdles for all such explanations—how does one use individual-level phenomena to explain social-level behaviors? The only answer—usually implicit—is that the social is merely an aggregate of the individual; ergo, a sick society is merely a collection of sick individuals.

There is reason to doubt that this particular image of the connection between individual and social phenomena actually works. In fact, it is more persuasive that *the social* forms the individual rather than the other way around. The other major problem is that purely psychological approaches tend to naturalize racist behavior; that is, the psychological mechanisms described in child development theories are generalized over all times, peoples, and places. As such, they become in reality (or function) like biological explanations.¹⁷ So in a period where we have finally come to see race as socially constructed rather than biological, we are asked to reverse field and explain racism in terms of innate human processes. Although Kovel and other proponents of such approaches would not deny that

social structures shaped individual psychologies, their explanations do not explain the exact relation between these two levels and thus imply a psychoanalytical remedy for racial ills. The implications of this for any theory of social change and for any effort to change society are very discouraging, to say the least.

Winthrop Jordan's *White over Black*, which attempts to explain the origins of American racism, reflects some of the conceptual and analytical difficulties encountered in the historical application to racial phenomena of the typical psychological approach.¹⁸ Much like Kovel, Jordan locates racism's origins in the derogatory attitudes that white people had toward blackness long before they encountered Africans. These attitudes were reflected in their language, which was emotionally coded to denigrate anything dark or black, and in the formation of their sexual personalities, whereby repressed fantasies were projected onto blacks. Thus the Elizabethan Englishmen's sexual anxieties and guilt, projected onto West Africans, gave shape to racial "attitudes," which were passed on to their English and American descendants. But since the subsequent generations, like Thomas Jefferson's, appear to have reproduced the original racist ideas in strikingly similar ways to the Elizabethans, history becomes more a backdrop for than a factor in the process Jordan describes.

As with other psychological explanations, Jordan's suffers from a lack of clarity as to the exact connection between the individual pathologies and the social action they purport ultimately to describe and explain. Jordan attempts to get around the problem by invoking parallel economic and social developments: that is, that Africans were enslaved because they were needed to work American sugar and cotton plantations; that English psychological pathologies regarding race were exacerbated or stimulated by societywide cultural anxieties during the age of discovery, and so forth. But in his explanatory schemes, all these phenomena are largely peripheral to and certainly are not the driving force behind the development of racist thought and action. Thus time and time again Jordan resorts to the sheer shock effect that the African's color purportedly excited and its resonance at the deepest levels of white psyches.¹⁹ This has the effect of "naturalizing" the process and thus cuts against his otherwise commendable efforts to historicize it. Consequently, despite his gestures toward material or sociological explanations, Jordan's analysis relies at its base on the mechanism of differentiation and projection found in individual, unconscious minds, that is, on their innate properties. We are left with the inference, for example, after the long exploration of Thomas Jefferson's psychosexual problems, that we need only project Jefferson's mind onto a larger screen to have the American mind, or at least the white male version of it.

How then do we get from the individual to the social level of analysis? Clearly the kind of structural analyses necessarily involved in any economic or materialist explanation tend to rely too much on rational-conscious motivations, or they downplay human agency and become too schematic, even deterministic. Also, such approaches cannot account for the irrational, the unconscious aspects so prevalent in the long history of race in America. Psychological explanations, on the other hand, can be enlightening about how racial notions function in individ-

ual pathology but seem incapable of explaining collective acts, except as aggregates of individual tendencies. In either form, deterministic schemes lose contingency and complexity, a sense of historical development, and racism tends again to become naturalized. What is needed, then, are explanations that are symmetrical at both levels—that is, where the connections between individual thought, belief, and action can be related to or explained in relation to societywide phenomena and vice versa.

The Cultural Paradigm. My notion of a “cultural paradigm” is a loosely framed scaffolding over a broad terrain. By this rubric I intend to convey some sense of works that share certain premises: Racism is a product of historically specific social formations. It is neither exogenous to the society (i.e., coming from the outside) nor reducible to or the effect of something else (like class). Rather it is a part of a given culture, often in some sense, simultaneously a product of the culture and producing that culture. (Indeed, one of the dangers of some variants of this approach is that they tend to totalize racial phenomena or culture or both).²⁰

Ronald Takaki's *Iron Cages* is one of the more stimulating and provocative early works within this genre. Takaki attempts to locate the development of mature American racism in the evolution of American culture during the nineteenth century. The American Revolution ruptured the moorings that held the former British colonists to a secure sense of self, place, and destiny, even as it freed them to pursue their manifest destiny of material and geographical expansion. In the absence of aristocratic hierarchies, social relations were more thoroughly mediated by the market, which allocated assessment of spiritual value and self-worth as well as material rewards and punishment. Material and moral success now depended transparently on self-discipline and self-denial. Drawing on Max Weber, Takaki argues that such a social regime must perforce foster repressed anxieties and guilt, the outlet for which was projection onto the colored minorities, who were deemed to harbor all the repressed sins of the white Other.

In the North and the South, the racial ideology of the black “child/savage,” in its emphasis on the need to develop self-restraint and accumulate goods, complemented the ideology of capitalism and gave specific support to Jacksonian individualism and enterprise. . . . [T]he black “child/savage” represented what whites thought they were not, and more importantly—what they must not become. . . . In the total structure of American society, racial and class developments interpenetrated each other. White over black had an organic relationship to class divisions and conflicts forming within white society.²¹

Although the structure of Takaki's argument is very similar to that within the psychological paradigm, its substance and media are less psychological than cultural. Culture spawns and sustains political, economic, and social institutions; it is also the product of such institutions. Indeed, Takaki conflates culture with ideology, defining it as “a shared set of ideas, images, values, and assumptions about human nature and society,” and his access to it is through the cultural

productions of the elite, what he calls “the culture-makers.”²² Thus the problems raised by Takaki’s analysis are general to this genre of historical explanation.²³ What is the relation between culture and material life? Indeed, just what is culture? What is ideology—and its relation to culture and material life? How can we most usefully think about the nature of hegemony and agency, or the roles of elites and masses in shaping racial phenomena?

In sorting out these questions as they emerge in the historiographical literature on race, it might be useful to recognize the distinctions between and the interdependence of the three concepts: culture, ideology, and discourse. Although there are many possible definitions of these concepts, for our purposes *culture* might be taken quite simply as the way we live, or rather the practices by which we live (doing); *ideology* as the way we understand how we live (knowing); and *discourse* as the way we communicate those understandings (making known). The cultural is intimately linked to both our material and nonmaterial lives—our economy, our various power structures, our technologies, as well as our social and spiritual life. It involves all those systems that mediate our relations with other human beings and with the natural world. The ideological comprehends all the ways in which we understand, mentally order and reorder, manipulate, or visualize those dense systems or webs of interrelationships. The discursive invokes that complex system of symbols by which we code and thus are able to transmit what we know and what we feel. As such it is an essential window onto the ideological and the cultural landscape. It not only reflects meaning but creates it by forging new connections, metaphorical associations, and so forth. And most important, it is not just the language or verbal system but includes a whole array of nonverbal signs and symbols as well. All three concepts, then, are social and collective as well as individual. Without the individual level, they are deprived of life; without the social level, they are deprived of meaningful effect.²⁴ All three are connected in complex ways, therefore, but should not be conflated.

Some of the most recent work on racial and class formation in nineteenth-century America has begun to approach this more complex level of discussion.²⁵ In *Wages of Whiteness*, for example, David Roediger has demonstrated how “whiteness” and “blackness” were mutually constituted within the selfsame process by which a white working class was formed in nineteenth-century America. Although Roediger’s analysis bears a family resemblance to Takaki’s of that same period, in Roediger’s story the culture of white workers is not simply the artifact or by-product of white elite hegemony. White workers were subject to stresses and strains imposed by economic transformations raining down from above, but they also made their own decisions, adjustments, and mistakes. They created a vibrant vernacular language, lively popular theater, and street parades that were democratic outlets for their joys and grievances. But these same forms of discourse, culture, and ideology demeaned and stigmatized blacks—and often fostered direct physical attacks upon them. Most demeaning of all, perhaps, was blackface minstrelsy, which constituted at once a cultural institution, an ideological production, and a set of discursive practice that would stretch well into the twentieth century.²⁶

What Roediger's work—and the work of others along similar lines—suggests, therefore, are the ways that race and racism might be comprehended historically; that is to say that they are not just socially constructed but are historical processes as well. Humans make race, to paraphrase Marx, but they are not free to make it in any old way they please. The constraints within and the givens with which they construct racial meanings are not biological but historical, including the material conditions that history fashions. An historicized, social process allows space for agency and choice at the individual level, yet individual behavior does not arise out of some naturalized psychological processes but is “determined” in the arena of social relations.

Furthermore, this approach suggests that we need to conceptualize racist practice in relation to all manner of other ordinary human intellectual, cultural, and social practices. Thus racism is not seen simply as some kind of abnormality, outside the realm of ordinary affairs, a historical wrong turn. Produced in the social world, its potential is ever present. Recognition of all this might displace the persistent tendency to biologize race and the reaction to race, locating both somehow in our genetic makeup. To say that these phenomena are profoundly historical is also to attempt to reconcile constraint with volition, and the ideological with the material.

What the historiography of racism in American history suggests, therefore, is that an effective analysis of racial phenomena must be at once psychological and sociological, material and nonmaterial. This is not to suggest some mere eclectic mix of approaches, however, but a reconceptualization of the very meaning of individual action, that under certain circumstances the dichotomies between individual and social, material and nonmaterial are false ones. Individual preferences, values, ideas, behaviors have meaning only in a social context; they are constituted out of the social and are in large measure the effects of relations of power. Put another way, the multiple exercises of power create the individual; and individual subjects are both the objects of power and its conduits. Thus how we understand—and explain—racism depends very much on how we understand social action more generally; and how we understand social action in general will be powerfully informed by how we understand racial phenomena.

NOTES

1. W.E.B. Du Bois, *Dusk of Dawn: An Essay toward an Autobiography of a Race Concept* (1940; reprint, New York, 1975), 51, 58, 67.

2. This chronology is implicit, if not explicit, in most contemporary analyses of racism. For its most explicit form, see Ivan Hannaford's *Race: The History of an Idea in the West* (Baltimore, 1995), which argues the thesis of racism's modernity in exhaustive detail.

3. For a sharp critique of these tendencies, see especially Barbara Fields, “Ideology and Race in American History,” in *Region, Race and Reconstruction: Essays in Honor of C. Vann Woodward*, ed. J. Morgan Kousser and James McPherson (New York, 1982); and idem, “Racism in America,” *New Left Review* 181 (May/June 1990): 95–118.

4. Here I am thinking in particular of the work, among others, of Stuart Hall's "Race, Articulation and Societies Structured in Dominance," in *Sociological Theories: Race and Colonialism* (Paris, 1980); Paul Gilroy's *There Ain't No Black in the Union Jack* (Chicago, 1991); David Theo Goldberg, *Racist Culture: Philosophy and the Politics of Meaning* (New York, 1993); and Michael Omi and Howard Winant, *Racial Formation in the United States: From the 1960s to the 1980s* (New York, 1986).

5. For examples in the recent literature on France, see Pierre-André Taguieff, *La force du préjugé: essai sur le racisme et ses doubles* (Paris, 1987); Maxim Silverman, ed., *Race, Discourse and Power in France* (Aldershot, 1991); idem, *Deconstructing the Nation: Immigration, Racism and Citizenship in Modern France* (London, 1992); Colette Guillaumin, *Racism, Sexism, Power and Ideology* (London, 1995); and Etienne Balibar and Immanuel Wallerstein, *Race, Nation, Class: Ambiguous Identities* (London, 1991).

6. Reginald Horsman, *Race and Manifest Destiny: The Origins of American Racial Anglo-Saxonism* (Cambridge, 1981).

7. *Ibid.*, 4.

8. *Ibid.*, 5 (emphasis added).

9. *Ibid.*, 189–207. Cf. Ronald Takaki, *Iron Cages: Race and Culture in Nineteenth-Century America* (New York, 1979), 55–65, 80–107.

10. Alexander Saxton, *The Indispensable Enemy: Labor and the Anti-Chinese Movement in California* (Berkeley, 1971).

11. *Ibid.*, 261.

12. Ironically, in parts of the postbellum lower South—where they were a plurality or even majority in some instances—blacks were sometimes able to forge temporary alliances with white worker organizations in shipyards and plantations. For example, Eric Arnesen, *Waterfront Workers of New Orleans: Race, Class and Politics, 1863–1923* (New York, 1991).

13. Cited in Thomas C. Holt, "The Political Uses of Alienation: W. E. B. Du Bois on Politics, Race, and Culture, 1903–1940," *American Quarterly* 42 (June 1990): 313.

14. Edmund S. Morgan, *American Slavery, American Freedom: The Ordeal of Colonial Virginia* (New York, 1975).

15. I do not mean to deny the potential usefulness of psychoanalytic paradigms as such. Indeed, some feminist theory may yet demonstrate the general applicability, for example, of Lacanian thought to problems of difference, which might include race. But these perspectives have not to my knowledge been systematically and effectively applied to racial phenomena as yet.

16. Joel Kovel, *White Racism: A Psychohistory* (New York, 1971), 46–105.

17. For examples, see Colette Guillaumin, "'Race' and Discourse," trans. Claire Hughes, in Silverman, ed., *Race, Discourse and Power in France*, 5–13; and George W. Stocking, Jr., "Essays on Culture and Personality," in Malinowski, Rivers, Benedict and Others: *Essays on Culture and Personality*, ed. George W. Stocking, Jr. (Madison, 1986), 5.

18. Needless to say, perhaps, this is not a claim that Jordan was influenced by Kovel; Jordan's book was published first. Winthrop Jordan, *White over Black: American Attitudes toward the Negro, 1550–1812* (Chapel Hill, 1968).

19. For examples, see Jordan, *White over Black*, 5–7, 95–97, 142–44, 257, 341, 458–59, 475.

20. An example of this totalizing effect is Omi and Winant's *Racial Formation*, in which we are told to think of "race as an unstable and 'decentered' complex of social meanings constantly being transformed by political struggle," and told at the same time that "the racial dimension [is] present to some degree in every identity, institution and social practice in the United States" (68). In short, the racial formation appears to include everything

and to be everywhere. Thus it becomes just a covering term, not an explanation of relationships or processes.

21. Takaki, *Iron Cages*, 126, 127.

22. "What white men in power thought and did mightily affected what everyone thought and did." *Ibid.*, xiv–xv.

23. Another notable work within this genre is Joel Williamson's *Rage for Order: Black-White Relations in the American South since Emancipation* (New York, 1986). Williamson explains the resurgence of what he calls southern "white radicalism" in the 1890s by a combination of political and economic interests that blacks directly or indirectly threatened and the conjuncture of general social and psychological anxieties in which sexual and racial fears were aroused by the sudden inability of white men to fulfill their gender roles as economic providers for their women and families. There followed what might be best described as a generalized social psychosis in which "the rage against the black beast rapist was a kind of psychic compensation." A more subtle and nuanced elaboration of the general scheme Williamson frames can be found in Glenda Elizabeth Gilmore, *Gender and Jim Crow: Women and the Politics of White Supremacy, 1896–1920* (Chapel Hill, 1996). See also Nancy MacLean, *Behind the Mask of Chivalry: The Making of the Second Ku Klux Klan* (New York, 1994).

24. Cf. Henri Lefebvre, *Critique de la vie quotidienne: Fondements d'une sociologie de la quotidienneté* (Paris, 1961), 143–44. See discussion of this text in Thomas C. Holt, "Marking: Race, Race-Making, and the Writing of History," *American Historical Review* 100 (February 1995): 1–20.

25. The works I have in mind are David R. Roediger, *The Wages of Whiteness: Race and the Making of the American Working Class* (London, 1991); Alexander Saxton, *The Rise and Fall of the White Republic: Class Politics and Mass Culture in Nineteenth-Century America* (London, 1990); and Eric Lott, *Love and Theft: Blackface Minstrelsy and the American Working Class* (New York, 1993). My discussion of these texts will be brief and wholly inadequate here, but I have discussed them at greater length and more critically in Holt, "Marking," and in "Racism and the Working Class," review essay, *International Labor and Working-Class History* 45 (spring 1994): 86–95.

26. Holt, "Marking," 16–18.

Crèvecoeur's Question

HISTORICAL WRITING ON
IMMIGRATION, ETHNICITY,
AND NATIONAL IDENTITY

PHILIP GLEASON

THE STUDY of immigration and ethnic groups in the United States, as John Higham observed in 1982, “has long been an expression of a fundamental debate over the nature of American society.” Thanks to the renewal of large-scale immigration in the past thirty years, and to the recent emergence of “multiculturalism” as a way of understanding American society, we are now in the midst of a particularly intense phase of that debate. But the basic issue was formulated as a question more than two centuries ago. In his *Letters from an American Farmer* (1782), J. Hector St. John de Crèvecoeur asked, “What then is the American, this new man?” Today, we would use gender-inclusive language, and speak perhaps of “identity,” but the same underlying question fuels the debate: “What does it mean to be an American?”¹

Immigration figured prominently in Crèvecoeur's treatment of the question, and he gave it a very definite answer. The favorable conditions that obtained in American society—most notably, freedom and material well-being—were, he asserted, “melting” European newcomers of diverse backgrounds into a “new race of men” which was destined to achieve great things. This optimistically assimilationist interpretation of American nationality, though challenged by Know-Nothing nativism in the 1850s, dominated American thinking on the subject for a century. By 1900, however, immigration was widely regarded as a major social problem, and a movement to restrict it had come into being. After World War I, immigration was cut back sharply by the passage of laws that embodied invidious racial assumptions about the intrinsic worth of different nationality groups and whether they were “assimilable”—that is, whether they had it in them to become real Americans. From the midtwenties, when restriction took effect in earnest, until 1965, when a new law changed the basis of American policy, immigration seemed a thing of the past. Since then, it has come back strongly as a social reality and an issue of public policy.

Our concern in this essay is with the way professional historians have dealt with immigration, and (implicitly if not explicitly) with its relation to American national identity. The subject can be divided into four chronological phases, the

opening and closing dates of which are of course somewhat arbitrary. The first period begins with the founding year of the American Historical Association and ends with the appearance of the first general history of American immigration written by a professional historian.

THE PREHISTORY OF IMMIGRATION HISTORIOGRAPHY, 1884–1926

It is a striking coincidence that history established itself as a professional academic discipline at roughly the same time the “immigration problem” became a significant policy issue. Even more striking is the fact that professional historians paid almost no attention to immigration, either as an area of research or as a contemporary social problem. Only forty-nine doctoral dissertations on matters related to immigration were written between 1885 and 1920, and students of history produced less than a third of this meager total. Edward N. Saveth, who wrote the standard work on the subject, says that the first two generations of professional historians either passed over immigration in silence or “treated it as a sort of historiographic hangnail.”²

Preoccupied in the post–Civil War years with the theme of national unification, historians of that era focused on the nation as a whole, giving special attention to the constitutional and political aspects of national development. This helps explain why immigration seemed to them a side issue, but their attitude was surely reinforced by the fact that they were themselves of “old American stock” and took it for granted that the Anglo-Saxon or (in Henry Adams’s case) Norman strains were the ones that really counted in the making of America. Looking back on their work from the perspective of the 1990s, one might say that their outlook implicitly betrayed something of the “filiopietism” for which Saveth reproached the “amateur” (i.e., nonprofessional) historians of the day, who, along with social scientists, produced most of what was written on immigration, but who wrote as more or less militant champions of the ethnic groups whose stories they were telling. Saveth did not take note of filiopietistic tendencies on the part of mainstream historians. The reason, perhaps, was that though he lamented their neglecting immigration, he shared the academic professionals’ conviction that the national story constituted the larger whole into which subsidiary reports on the place of immigrants in American life were to be fitted.³ However that may be, we must postpone further discussion of filiopietism lest we get ahead of our own story.

After the turn of the century, historians’ work reflected in an incidental way the prevailing concern about immigration as a social problem, but the most explicit discussion by a leading historian, Frederick Jackson Turner, was confined to a series of newspaper articles that were largely descriptive in nature. Except for a book by the labor economist-cum-historian John R. Commons (who is not included among the professionals discussed by Saveth), they did not make extended contributions to the discussion. The same is true of the racist assumptions so prevalent among commentators on the immigration problem—

historians may have accepted racialism, but they were not the leaders in elaborating or promoting it. Elsewhere on the conceptual front, new terms of discourse were introduced that have retained their prominence to the present day, most notably “melting pot” and “cultural pluralism.” Neither was introduced by a historian. The playwright Israel Zangwill put the first in circulation; the social philosopher Horace Kallen, the second.⁴

None of the general histories of immigration appearing in those years was written by a professional historian; a social worker, Edith Abbott, edited the most important collections of documents; and the best known studies of individual groups were done by nonhistorians. Consider three books that merit being called classics: *Our Slavic Fellow Citizens* (1910) was written by Emily Greene Balch, an economist and social worker; *The Italian Emigration of Our Time* (1919), by Robert F. Foerster, an economist; and *The Polish Peasant in Europe and America*, 5 vols. (1918–20), by W. I. Thomas and Florian Znaniecki, two sociologists. It is true that Arthur M. Schlesinger, the pioneer of American social history, published an essay in 1921 calling his colleagues’ attention to the importance of immigration as a factor in the nation’s development. But it is revealing that the article appeared in the *American Journal of Sociology* before being included in Schlesinger’s *New Viewpoints in American History* (1922).⁵

Schlesinger’s being of immigrant background himself perhaps had something to do with his conviction that the Anglo-Saxon heritage was giving way to “a new composite American type now in the process of making.” The experience of World War I also played a role in alerting historians of the United States to its connections with the larger world. Even Turner expanded his horizons, “dimly discern[ing],” as Moses Rischin puts it, “a pattern that would place the immigration story within an international framework of frontier and section. . . .” As we have seen, Turner was not altogether unmindful of immigration before the war. He had, after all, grown up among immigrants in Wisconsin and often referred to the frontier as a crucible of assimilation. One of his early graduate students, Kate A. Everest, researched the coming of the Germans to Wisconsin in a dissertation that was the second immigration-history Ph.D. ever done at an American university. It should also be noted that Turner was *Doktorvater* to two other key figures in immigration historiography—George M. Stephenson, whose general history marks the end of the first phase of our story, and Marcus L. Hansen, the giant of the next phase, who encountered Turner just as the war opened the latter’s eyes to the possibility of “national cross-fertilization.”⁶

Stephenson launched his career with a doctoral dissertation on a classically Turnerian topic, public land policy. It was published as a book the year the United States entered the war, and the wartime experience probably helped to turn Stephenson’s attention to immigration. The nationalistic passions it aroused, among old-stock Americans as well as those of immigrant background, brought out with unprecedented clarity what he called the “problems of a composite citizenship.” He devoted two chapters of his *History of American Immigration* (1926) to the war and its effects, one being the Johnson-Reed Act of 1924, which

ended a century of largely unrestricted immigration and thereby, as Stephenson put it in the first sentence of the book, "closed a momentous chapter in American and European history, and indeed in world history."⁷

Apart from its landmark status, Stephenson's book holds up moderately well as a first effort to synthesize the history of immigration. It is very sketchy by later standards, but does include a relatively full and sympathetic discussion of "oriental immigration," which, as Stephenson acutely observed, "profoundly affected our whole immigration policy."⁸ By far the most striking feature of the book is its narrowly political focus. Aside from brief remarks on old-country backgrounds in Europe, Stephenson did not discuss immigrant groups as entities in themselves, focusing rather on how they impinged on U.S. national politics. Besides the participation of immigrants in party battles, he covered nativist reactions and the evolution of immigration policy. But only in respect to politics did immigration seem to function as a factor in American life. Stephenson, who was of immigrant background himself, did not lack interest in the internal history of immigrant groups, nor in the nonpolitical dimensions of their experience. Indeed, his next book was a detailed analysis of the religious history of his own group, Swedish Americans. But when he undertook to synthesize the history of immigration as a whole, his approach resembled that of earlier scholars who had neglected the subject, in the sense that he cast the story in terms of American political history as traditionally understood.

IMMIGRATION HISTORY BECOMES A FIELD OF SPECIALIZATION, 1926–1940

The first group of professional historians to specialize in immigration history were midwesterners of northern European derivation.⁹ Stephenson belonged to the group, as did his Norwegian-American colleague Theodore C. Blegen; together they made the University of Minnesota a major institutional center for immigration history. Carl Wittke, who was of German background, had a Harvard degree but spent his career as a teacher, writer, and administrator in his home state of Ohio. Marcus Hansen, whose father was Danish and mother Norwegian, was born in Wisconsin and educated in Iowa before going off to Harvard to study with Turner in 1917; he taught at the University of Illinois for the last ten years of his tragically short life, dying in 1938 at the age of forty-five. These were not the only historians interested in the subject, but aside from W. F. Adams's book on emigration from Ireland and R. A. Billington's study of antebellum nativism, they produced the outstanding works of the period in question.¹⁰

The social and cultural context within which they worked had changed notably from that of the preceding period.¹¹ The "immigration problem" had disappeared as a public issue; nativist fears evaporated after the religiously divisive presidential campaign of Al Smith in 1928; and "culture" (as anthropologists used the term) replaced "race" as the key to understanding human groups. Among second- and third-generation immigrants—that is, the children and

grandchildren of those who actually immigrated—assimilation seemed to contemporary observers to be proceeding very rapidly. After the depression hit, economic and political issues dominated the scene, but Franklin D. Roosevelt's New Deal encouraged a spirit of social liberalism and tolerance. Although intellectuals deprecated nationalism, by the late thirties the mounting threat of totalitarianism in Europe sparked an ideological revival of democracy that reinforced preexisting currents of interest in the roots of American culture and its manifestations in art, literature, and folklore. Social history, which had gained at least a foothold among professionals, reflected something of the same spirit, since its devotees sought to illuminate the ways of life of ordinary Americans.

Though immigration history was still a very minor theme, these developments created a more favorable climate for its reception. Three of its four major practitioners focused on the internal history of immigrant groups. Stephenson's 1932 study, which concentrated on the "religious aspects" of Swedish immigration, went far toward providing an overall history of the group, including the old-country background. Blegen's two-volume work on Norwegian immigration was even more comprehensive, and the publications of the Norwegian-American Historical Association, of which he was the driving force, set a new standard of scholarly excellence for such organizations.¹² Wittke, the most prolific immigration historian of his generation, was an indefatigable researcher who mined the ethnic press as no one had before. Though he devoted most of his scholarly attention to German-Americans, his published work covered a broad range and included a general history of immigration that served for two decades as the standard survey of the subject.

The appearance of Wittke's *We Who Built America* (1939) coincided with the reawakening of democratic nationalism set off by the outbreak of war in Europe, and its very title claimed for immigrants a place of honor in the making of America. Like his contemporaries, Wittke believed large-scale immigration was over, and he anticipated continued "fusion of [the] immigrant strains" already present in American society. But this basically assimilationist outlook did not justify overlooking the historic significance of immigration. Adducing a personal example, Wittke spoke movingly of his father, who had immigrated from Germany in 1889 and lived for almost half a century in the United States. The elder Wittke was a "thoroughly trained mechanic," whose simple but productive life, along with his devotion to American ideals, enabled him to "blend into the American stream" and provide for his children advantages he had not himself enjoyed. Of such "humble but honorable fragment[s]," his son declared, "the real Epic of America must eventually be written."¹³

Wittke disclaimed doing more than sketching the broad outlines of that epic story, but what he really supplied was an anthology of ethnic sub-epics. His book provides a great wealth of information on a score of immigrant nationalities. It is richest for the "old immigrants," especially the Germans and Irish, much thinner on Asians and the "new immigrants" from southern and eastern Europe whose history had not yet been studied in detail. The approach is, however, purely

descriptive. *We Who Built America* has no overarching interpretive scheme or principle of structural unity. Although he assumed, “of course,” that “our American civilization is basically Anglo-Saxon” and that the great majority of the people were English in derivation, Wittke nevertheless confined his attention to “non-English immigrants.”¹⁴ By doing so he presumably meant to highlight the role of these groups and compensate for their previous neglect. But since the Anglo-Saxons had supposedly shaped American civilization, omitting them left Wittke without a framework of national development into which “the saga of the immigrant” could be integrated. The result is a series of disconnected stories that fails to cohere as a unified narrative and leaves the reader without the sense of having grasped the subject as an intelligible whole.

Wittke’s attempt to write a comprehensive history of immigration by telling the story of one group after another dramatized the limitations of the internalist approach. Marcus Hansen had already expressed his dissatisfaction with it, even when restricted to the history of only one immigrant nationality. Reviewing Blegen’s first volume on Norwegian immigration in 1932, Hansen praised its merits and then went on to say: “But the student of American history is not interested primarily in nationalities. The significance of the movement [immigration] is broader than the experience of detached groups.” Historians should seek rather to understand “the mysterious forces that, disregarding political boundaries, operated to set mankind in motion” all across Europe, in one region after another.¹⁵ Precisely this was the task that Hansen set for himself, first in his doctoral dissertation, then in three additional years of research in Europe. Death intervened before he was able to finish even the first of the three volumes he envisioned. *The Atlantic Migration, 1607–1860* (1940), which appeared posthumously thanks to the editorial work of Arthur M. Schlesinger, is thus but a fragment of the full project.¹⁶

Truncated though it is, Hansen’s work constituted a monumental conceptual breakthrough. Prescinding altogether from nationalities as such, Hansen linked the whole phenomenon of immigration to broader historical developments affecting both Europe and America. Moreover, he showed that the forces at work in the migration of English settlers to America as “colonists” were the same as those that moved non-English “immigrants” in the colonial era and in the nineteenth century.¹⁷ By framing the story of colonial beginnings in this manner, and by showing that a sharp falling off of immigration between 1789 and 1815 hastened the assimilation of the immigrants already here, Hansen succeeded in linking immigration to the narrative core of American history as it had never been done before—and this despite his book’s being much more a study of emigration from Europe than of immigration to America. For Hansen perceived that immigration *connected* America to Europe and had to be studied at both ends. Since the process began with people being set in motion from somewhere, one should begin at that beginning, asking what unsettled them. From there, one followed along, inquiring what attracted them elsewhere, how they learned about such possibilities, how a multitude of political, economic, commercial, and

technological factors interacted in their movement and in their resettlement and adjustment in new homes. All this is now familiar, as is Hansen's sketch of demographic, agrarian, and industrial change moving across Europe from west to east, affecting one national group after another and sending them in successively mounting waves to the shores of America. But by comparison to the existing literature on immigration when his book appeared, it was a novelty of Copernican proportions.

Besides his pathbreaking analysis of the process of migration and the "mysterious forces" that shaped its operation, Hansen is also remembered for a collection of interpretive essays entitled *The Immigrant in American History* (1940). Since it appeared, two other essays have come to light, one of which was not uncovered until 1979 and not published until 1990.¹⁸ The essays sound more than one theme, to be sure, but they also establish pretty clearly that Hansen shared the liberal assimilationist outlook that characterized enlightened commentary on intergroup relations in the 1930s. According to this view, which was really a generous version of the melting pot, the existence of a distinctive American national culture is taken for granted. It had been shaped in fundamental ways by its English founders, but non-English ethnic groups had made their own unique contributions to it, even as they were being blended into it. Like other observers, Hansen lamented that the pragmatic and unimaginative host society had not absorbed many of the artistic and cultural riches the immigrants had to offer. But he was under no illusion that immigrant cultures were going to survive in America.

It is true that Hansen speculated that "the principle of third generation interest" might lead the grandchildren of the immigrants, who felt at home in America, to identify more closely with their ancestral heritage than did their second-generation parents, who (it was thought) were intent on throwing off the taint of foreignness. But he did not speak of third-generation interest as a force that could perpetuate the culture of the group as a coherent entity in itself. Rather, he asserted, "men of insight . . . understand that it is the ultimate fate of any national group to be amalgamated into the composite American race. . . ." All that could be hoped for was that third-generation interest might be directed toward a selective retrieval of "those features of [the immigrant group's] cultural life that should be added to the heritage of America." The most concrete form such a contribution could take was in the writing of history, and Hansen laid down firm guidelines for how it should be done. To bear good fruit historically, the third generation's delving into the ethnic past must follow two fundamental principles: first, the history of the group must be written without "self-laudation" and on "broad impartial lines"; secondly, it must be "made to fit in as one chapter in the larger volume that is called American history. . . ."¹⁹

Had he lived longer, Hansen's thought might have moved in more up-to-date "multicultural" directions.²⁰ As matters stand, however, we can confidently conclude that he accepted the benign version of melting-pot assimilationism characteristic of his day. And he obviously endorsed the idea of a "master narrative" for American history, though he did not, of course, call it that. We must, however,

add that his own historical work served not merely to enrich “the heritage of America” but also to reshape our understanding of “the larger volume that is called American history.”

THE HANDLIN EPOCH, 1941–1964

The next phase of immigration historiography is dominated by one man to a greater degree than any other period. Oscar Handlin gained instant visibility in the profession with the publication in 1941 of his doctoral dissertation, *Boston's Immigrants*, which won a major prize from the American Historical Association. Ten years later, *The Uprooted*, a Pulitzer prize winner, gained a wide general readership and made Handlin one of the best-known American academics of his time.²¹ He attracted a galaxy of brilliant graduate students and made Harvard the center for the historical study of immigration (though many of his students wrote on other subjects, for Handlin was very much a generalist). His influence continued long after the date given above as marking the end of the Handlin epoch. Indeed, a paperback edition of *Boston's Immigrants* went through nine printings between 1968 and 1974. But the publication in 1964 of Rudolph J. Vecoli's critique of *The Uprooted* foreshadowed the opening of a new era in immigration historiography in which Handlin's approach was, if not flatly rejected, at least severely discounted.²²

Of course everything was changing by the midsixties, when a new generation of young people rejected the world of their parents.²³ Handlin was very much part of that parental world—the world usually labeled “Cold-War America.” The label is misleading, because the defining experience for that generation of Americans was not the “Communist threat,” despite the disproportionate anxieties it aroused. The Cold War was but a protracted sequel to the real war—World War II—in which the United States was actively engaged for four years and the coming of which affected society and thought even before hostilities broke out in 1939. The experience of World War II was what shaped the thinking of Handlin's generation and carried over into the presidential administration of the navy veteran John F. Kennedy.

The awakening of national feeling and the ideological revival of democracy already discernible in the late 1930s were powerfully reinforced by the attack on Pearl Harbor and America's active entry into the struggle against totalitarian aggression. National unity became a wartime imperative, but it was to be based on the commitment of all Americans to a commonly held set of ideas. Because they all believed in freedom, equality, and the dignity of the individual—the most basic elements of the “American creed,” as Gunnar Myrdal called it—Americans were *one people* no matter what their race, religion, or national origin. These factors were, or should be, irrelevant to one's being an American. And since “tolerance for diversity” was a major corollary to the democratic creed, failure to regard racial, ethnic, or religious differences as irrelevant constituted a deviation from true Americanism, which, if it translated into prejudiced attitudes and

discriminatory behavior, amounted to a betrayal of democracy itself—a point driven home by the monstrous example of Nazi racism.

Besides defining wartime unity, democratic universalism underlay the postwar campaign to improve intergroup relations, the most significant aspect of which was the drive for desegregation and civil rights for African-Americans. It likewise played a role in the anticommunist hysteria, since communism represented a challenge to the very core of American national identity, that is, agreement on basic political principles. And it was obviously central to the “consensus” mentality said to characterize so much historical work of the 1950s, including that of Oscar Handlin. From the viewpoint of immigration historiography, however, the main point is that democratic universalism tacitly assumed a degree of immigrant assimilation that approached total absorption.

For if, despite our much-valued diversity, we were really “One America,”²⁴ it had to be because diversity didn’t cut very deep. And where it did cut deep, it created problems. Why, otherwise, was “divisiveness” such a pejorative term if “pluralism” was such a good thing? Actually, “cultural pluralism” as it was understood at midcentury differed little in substance from the traditional assimilationist belief that many ethnic elements had contributed to, and were blending together in, a composite American nationality. In fact, the individualistic basis of democratic universalism implicitly required assimilation. The connection was not developed at the time; perhaps it was not even recognized in the rhetorical climate of tolerance-for-diversity. But to improve intergroup relations one had to eliminate sharp group differences, whether based on race, religion, or some other cultural feature. Racial integration was a goal clearly assimilationist in tendency; Nazi racism, of which Jews were the principal victims, dramatized how religious divisions could be exploited. Group consciousness and group pride, far from being prized as healthy signs of “ethnicity” (a term that had not yet come into general use), were branded “ethnocentric”—an attitude that, according to one influential school of thought, reflected a fascist mindset.

In its linkage with democratic universalism and desirable social goals, assimilation was tacitly accepted as a good thing. But it was not an unmixed blessing. Indeed, it was more likely to be thought of in connection with something that aroused widespread concern at midcentury—the fear that America was fast becoming a “mass society,” whose remaining diversity was destined to disappear in a sea of bland “conformity.” Mass society had many sources. At bottom were the dislocations brought on by industrialization, urbanization, and modernization. However, Tocqueville (who enjoyed a great revival in these years) had pointed out a century earlier that democracy itself contributed to the process. Equality, which it prized above all, eroded traditional social groupings and nurtured a spirit of individualism that isolated people from one another. A society reduced to nothing but a dust of individuals could hardly resist what Tocqueville called the tyranny of the majority. Since his time, new media of communication made the problem more urgent, for Hitler had shown how easily the deracinated masses could be manipulated. Hitlerism had been defeated, but the war left deep psychic scars—to which were added the Cold War and the prospect of nuclear

destruction as sources of continuing anxiety. Small wonder that postwar commentators found many disturbing symptoms of anomie, alienation, and need-for-belonging in the "lonely crowd" that constituted American society.

Against this complex background—an "American celebration" troubled by undercurrents of ambivalence, irony, and ambiguity—*The Uprooted* appeared in 1951. Its title immediately contributed a new metaphor for the prevailing sense of malaise, to which Handlin was keenly attuned. The book likewise reflected his sophisticated acquaintance with the social sciences, something already evident in *Boston's Immigrants*, which was subtitled "a study in acculturation." Having grown up in New York City, the son of Russian Jewish immigrants, Handlin brought to his work a perspective derived from personal experience quite different from that of the midwestern pioneers of immigration historiography. But like Wittke and Hansen, he wanted to tell the full story of immigration and demonstrate its centrality to American history. "Once I thought to write a history of the immigrants in America," reads the first line of *The Uprooted*. "Then I discovered that the immigrants were American history."²⁵

To illuminate the nation's past by following the single strand of immigration, Handlin essayed an interior history of the phenomenon, focusing his attention on the experience of the immigrants themselves. In telling the story, he drew at least as much on humanistic empathy as on his knowledge of social psychology, but to give it breadth of scope he generalized freely, adopting at times the sociologist's ideal-typical approach. The latter method is most evident in chapter one, "Peasant Origins," but Handlin aims throughout to capture the typical experience of immigration, merely taking note of the most significant variations. In this manner he moves from European background, through the ordeal of the ocean crossing, to a series of chapters covering the manifold dimensions of the immigrant's adaptation to life in the New World. The book also describes the host society's reaction to immigration and concludes with a meditation on the larger meaning of the immigrant experience.

The most striking feature of the book is the elegiac, almost lugubrious, tone that results from Handlin's unremitting emphasis on alienation, separation, sadness, and loss. Interpreting immigration as "a history of alienation and its consequences" spoke poignantly to the sensibilities of literate Americans at midcentury and shaped that generation's understanding of the immigrant experience. More than that, it opened a new perspective on the spiritual inquietude of the day because the immigrant's alienation was, in greater or lesser degree, the experience "of all those whom the modern world somehow uproots."²⁶

Unlike its transparent relevance to this dimension of the prevailing mentality, *The Uprooted* related more subtly to the contemporary emphasis on democracy with its tacit assumption of assimilation. Uprootedness as Handlin described it certainly ruled out the survival of coherent and permanently distinctive ethnic cultures in American society. He was, in that sense, an assimilationist. But he took assimilation as a given; he did not celebrate it. On the contrary, his book is an inventory of its psychic cost in "broken homes, interruptions of a familiar life, separation from known surroundings, the becoming a foreigner and ceasing to

belong.”²⁷ Yet there is a still deeper ambivalence, for the shock of alienation was at the same time a radical *liberation* which opened the way to existential nobility. Being torn from the nest of ancestral security and required to overcome one obstacle after another awakened in those who were uprooted a new consciousness of self. Though infinitely painful, their struggles drove them back upon themselves, thereby bestowing upon them “the human birthright of . . . individuality.” This experience prepared them for life in America, for displacement was a characteristic feature of this “land of separated men” and the immigrants had already learned that freedom exacted its price in personal suffering.²⁸

Handlin’s assimilationism was thus deeply ambivalent, his Americanism embedded in a tragic sense of life. But despite its pervading melancholy, *The Uprooted* affirmed the basic national values, freedom and the dignity of the individual. The same critically positive stance toward American life may be found in Handlin’s other works, but they are too numerous and wide-ranging for discussion here. Concentrating on *The Uprooted* is justified because it was so widely read at the time and because it is the book later critics have singled out as epitomizing the mistaken assumptions governing immigration historiography at midcentury. That does not mean of course that all other scholars patterned their studies upon it. The only other work of the fifties on immigration to attain classic status, John Higham’s *Strangers in the Land* (1955), approached the subject from a very different angle, using the prism of nativism to analyze its significance in American history. The British scholar Maldwyn Allen Jones’s admirable survey *American Immigration* (1960) owes at least as much to Hansen as it does to Handlin.²⁹ Even Handlin’s own students, who dominated the field, produced works too fresh and original to be thought of as mere repetitions of a model. It is, however, fair to say that immigration historians of that era accepted the view that assimilation was inevitable and had in fact taken place. They likewise tended to regard it benignly because “Americanization” (as it was often called) meant that the immigrants had been absorbed into a nation whose essential identity derived from common acceptance of praiseworthy social and political ideals. All these assumptions were to be called into question in the next epoch.

IMMIGRATION HISTORY IN ERUPTION, 1964–1997

Unparalleled growth in the number of works produced, in addition to sharp shifts in interpretive stance, make it only mildly hyperbolic to call the last three decades a revolutionary epoch in immigration history. When Rudolph J. Vecoli published the first of his valuable historiographic surveys of the subject in 1970, he reproached historians for having neglected immigration; before the decade ended, he found himself “inundated by a virtual flood” of new publications on race and ethnicity.³⁰ In 1970, Jones’s brief survey was the only thing approximating a suitable classroom text; since then, upward of a dozen textbooks or general syntheses have appeared—along with hundreds of monographs, major reprint

series, new scholarly journals, a new professional association, several centers devoted to research on immigration, and the landmark *Harvard Encyclopedia of American Ethnic Groups* (1980). Dramatic increases in the number of doctoral dissertations tell the same story: 314 were written on immigration or ethnicity in the 1950s; 677 in the 1960s; and 1,813 in the 1970s.³¹ Given the scale and complexity of the phenomena in question, what follows must be not only schematic but also somewhat speculative in character. Before turning to immigration historiography as such, however, we must look briefly at the larger context.

It is, in the first place, quite clear that the race issue and the Vietnam War, which together dominated the American scene in the 1960s, influenced the outlook of scholars interested in immigration.³² The middecade shift of emphasis from civil rights to black power coincided with large-scale American military involvement in Vietnam and the growth of a widespread and passionate antiwar movement at home. This combination—along with urban riots, campus disorders, the New Left, the counterculture, the women's movement, and a series of political assassinations—fueled a deep and pervasive radicalization of feeling. In the atmosphere of crisis that accompanied these developments, two shifts in perspective stand out as influences on subsequent historical scholarship. On the one hand, particularistic group consciousness, group pride, and group assertiveness—hitherto deprecated as ethnocentrism—were now *legitimated* as ethnicity. On the other hand, Americanism as it had been understood since World War II was *delegitimated*—discredited because racism at home and imperialism abroad persuaded the most radical critics that the “American creed” was a mere smoke screen for oppression, and left more moderate observers shaken and uncertain whether the nation's historic principles would endure.

Just before the midsixties shift in feeling took hold, a new immigration law abandoned the system in place for forty years whereby “national-origins quotas” determined the number of entries allocated to various countries. The rejection of this system, which was based on the invidious racist assumptions of the 1920s, paralleled the civil rights legislation passed at the same time and represented the high-water mark of the democratic universalist outlook of the post-World War II era. It also opened the way to an enormous expansion of immigration which, for the first time in the twentieth century, included Asians on the same basis as immigrants from the Western Hemisphere. The new law, along with several major refugee flows and heavy migration from Latin America, resulted in unanticipated changes in the ethnic composition of the immigrant population as well as its vast enlargement. Of the almost twelve million immigrants who entered the country between 1971 and 1990, over 70 percent were either Asian or Latin American; only about 13 percent were European. (These figures do not include an unknown but significant number of illegal, or “undocumented,” immigrants.)³³

By the 1980s, these developments made immigration and multiculturalism important matters of public policy; in the 1960s, however, the most obvious immigration-related issue involved Mexican-Americans. For despite their historic

roots in the Southwest, and a vast increase in their numbers brought about by immigration after 1910, Mexican-Americans did not gain real visibility on the national scene until the 1960s, when the agricultural workers' strike led by Cesar Chavez associated their cause with the broader civil rights movement. The subsequent emergence of a more militant "Chicano" movement reflected the midsixties shift. Thanks to the religious idealism of the earlier crusade led by Martin Luther King, Jr., "minorities" already held the high ground morally. In that context, the interrelated legitimization of ethnicity and discrediting of Americanism encouraged a new spirit of militance all along the line. "Black Power" gave rise to "Brown Power" and "Red Power," and eventually to a generalized "revival of ethnicity." The latter, also called the "new pluralism," drew upon residual group feeling among the descendants of European immigrants but was also to some extent a consciously devised strategy aimed at defusing the "backlash" thought to be building up among "white ethnics" resentful of the gains made by other minority groups, especially African-Americans.³⁴

Along with these societywide developments, revisionist stirrings in the academic world gave early warning of the coming historiographic upheaval. Even before the civil rights/black power shift, two books by social scientists—Nathan Glazer and Daniel P. Moynihan's *Beyond the Melting Pot* (1963) and Milton M. Gordon's *Assimilation in American Life* (1964)—challenged earlier views about the degree and depth of immigrant assimilation.³⁵ In 1964, Vecoli's previously mentioned critique of *The Uprooted* focused that challenge on the regnant historical paradigm. A revitalized labor history soon discovered the relevance of immigration, and the New Left perspective, which was strong among labor historians, made the unmasking of Americanist pretensions a congenial task. Political historians had begun to explore "ethnocultural" influences on voting behavior, while social historians—among whom Handlin's student, Stephan Thernstrom, was a pioneer—revealed that ethnicity was a significant variable in social mobility. Social mobility as a contemporary reality figured in the picture more directly, for the vast postwar expansion of higher education brought into academic life unprecedented numbers of graduate students of immigrant background. As this ethnically diverse cohort took its place in history departments, the combination of intradisciplinary and broader cultural shifts created the most favorable climate for the study of immigration history that had ever existed.

The outpouring of scholarship that followed defies comprehensive summary; its very richness makes any effort to identify the leading trends a risky business, for in expanding so rapidly immigration history lost much of its conceptual tidiness. The growing importance of Hispanics and Asians has added distinctive new elements; the refugee issue demands greater attention than in the past, as do considerations of gender and class. Topics hitherto regarded as belonging to specialists in other fields (e.g., African-American and Native-American history) are now often included in treatments of immigration history. This is most notable in works that make ethnicity their central organizing principle, for "ethnic history" is a more inclusive category than "immigration history." Indeed, the compilers of

a recent collection of course syllabi characterize the difference between the two approaches as “a central tension within the field” and hint at epistemological profundities.³⁶

Further conceptual ambiguities lurk in the terms “race” and “ethnicity”—and in the relationship between them. As a scientific idea, race has supposedly been discredited for more than half a century. David Hollinger accepts its discrediting but insists that “racism” is still a viable concept, though “race” isn’t—which hardly simplifies matters. And even if race is interpreted as a cultural construct rather than a biological reality, the question remains how it differs from ethnicity, the constructed nature of which has been emphasized by writers who speak of “the invention of ethnicity” and the process of “ethnicization.” The two concepts obviously cover much the same ground, but there is no theoretical consensus about their respective boundaries. Thus the *Harvard Encyclopedia of American Ethnic Groups* treated race as a dimension of ethnicity, but a reviewer prominent in the study of plural societies branded this a “monumental confusion” because it subsumed race under a conceptual category trivial by comparison.³⁷

Assigning greater theoretical importance to race is certainly in line with recent developments in the “real world,” the most important of which is affirmative action. This complex of policies, as it evolved in the late sixties, was designed with African-Americans in mind, yet the term used to designate those to whom it would apply was “minorities” (and under a separate provision, women). Because a multitude of groups had previously been included under that rubric, a short list of minorities eligible for affirmative action benefits had to be drawn up. What ultimately emerged from the bureaucratic mazes where this process was carried out was the now-familiar list—African-Americans, Native Americans, Asians and Pacific Islanders, and Hispanics. Since these groups were generally identifiable by physical differences popularly thought of as “racial,” and since African-Americans had unquestionably suffered from “racism,” the practical logic of the situation enforced the conclusion that something called “race” really existed. The corollary, upon which government policy seemed clearly to be premised, was that victims of “racism” deserve special status in the polity because the wrongs visited upon them were *different in kind* from the prejudice and discrimination suffered by others in American society. In the quarter century it has been in force, affirmative action has thus lent powerful impetus to the racialization of American thought—a fact illustrated by recent efforts by Mexican-Americans to get themselves explicitly classified as a *racial* group in the next federal census, and by the rise of “whiteness studies.”³⁸

Besides adding to the conceptual ambiguities of the field, affirmative action figures in another way in the complications that beset historians of immigration and ethnicity—it is highly controversial. So also is immigration policy itself, to say nothing of multiculturalism and associated issues of “political correctness.”³⁹ This does not mean that historians cannot continue their work; indeed, controversy makes their studies more timely and relevant. Yet the distortion and oversimplification unavoidable in an atmosphere of controversy exacerbated by

partisan politics cannot help but affect the realm of discourse in question. This might not turn historians themselves into partisans—although the ideal of scholarly detachment is not as robust as it used to be—but it will not lighten their task.

Despite all these ambiguities and complications, certain features of the new historiographic landscape stand in sharp contrast to what had gone before. The most striking is a twofold reversal of opinion on the subject of assimilation. *Sociologically*, according to the revisionists, the concept of assimilation is fundamentally flawed because the process it refers to didn't happen; *ideologically*, it must be rejected because it embodies an unacceptable vision of American society. These positions are not uniformly held by all historians, or equally emphasized by those who do, and several scholars have recently reaffirmed the reality of assimilation as a social process. Although the U.S. Commission on Immigration Reform has undertaken to rehabilitate "Americanization," its ideological status remains deeply suspect, and if we consider the last quarter-century as a unit, the twofold revision of the concept of assimilation has enjoyed hegemonic status. Both of its aspects were reflected in the all-but-universal execration of melting-pot symbolism, and the talismanic quality taken on by the terms "cultural pluralism" and, more recently, "multiculturalism" and "diversity."⁴⁰

Explicit discussion of interpretive issues, along with pejorative or honorific use of symbolic language, is most likely to be found in journalistic, polemical, or programmatic contexts, but the revisionist stance also shaped more technical scholarly work in immigration history. The title of a much-admired synthesis, John Bodnar's *The Transplanted* (1985), suggests the way Handlin's *Uprooted* has served as a foil in this regard. Handlin erred, say the revisionists, in portraying the immigrants as deracinated individuals, buffeted by impersonal forces beyond their control, whose pain and suffering in an alien land were prospectively redeemed by the better American future the more assimilated second generation would inherit. Actually, according to the new view, the immigrants were prepared for change by prior Old-World experience; their decision to emigrate was rational, freely undertaken, and occurred within a framework of family and communal support, as did the process of migration and resettlement. Far from being first uprooted and then assimilated, immigrants brought most of their ancestral culture with them, preserved their ethnic identity, and perpetuated their distinctive culture as part of the mosaic of a pluralistic society. So dominant had this revision become by 1990 that Vecoli, who first adumbrated it in 1964, warned historians not to substitute a new stereotype for the old "caricature of uprooted, oppressed, traumatized victims," and Jon Gjerde's recent study of nineteenth-century immigrants in the Midwest is, indeed, far more nuanced.⁴¹

The revisionist interpretation is based on thorough research and corrects exaggerations in the older view, especially Handlin's almost melodramatic emphasis on alienation. But as Vecoli's remarks suggest, it is subject to its own exaggerations, and it reflects changes in the overall ideological climate at least as much as it reflects the unearthing of new information or advances in analytical understanding. We may grant, for example, that simplistic versions of assimilation

went too far. But could the perduring quality of ethnic culture, which was treated as virtually axiomatic in the seventies and eighties, have attained that theoretical status absent the legitimation of ethnicity that took place in the sixties? And since assimilation implies identification by immigrants with the host nation's ideals and institutions, the discrediting of old-fashioned Americanism was a necessary precondition to the celebration of "unmeltable ethnicity." Thus Nathan Glazer could speak in 1975 of a situation in "the ecology of identities" that made it more advantageous to claim an ethnic identity than to affirm that one was simply an American.⁴² And though "multiculturalism" can mean many things, the strongest versions of the 1990s interpret traditional Americanism as oppression and deny that a straightforwardly "American" national identity even exists.

Another revision closely related to the uprooted-to-transplanted shift is the insistence that American immigration must be viewed within the context of larger international (primarily European) migration patterns. Though Hansen's work pointed in this direction, the decisive stimulus came from a paper delivered at the International Congress of Historical Sciences in 1960. There Frank Thistlethwaite, a British scholar who spent two years at the University of Minnesota in the late thirties, drove home the point that overseas migration had to be set against the background of intense prior and contemporaneous *intra-European* migration.⁴³ It took time to lift what Thistlethwaite called the "salt-water curtain," but by the mid-1970s several European-based migration research projects were under way, and American studies like Josef Barton's *Peasants and Strangers* rested on intensive analysis of the old-country matrix from which overseas migration sprang. Several more recent works demonstrate that the transatlantic perspective is now firmly established and illustrate the ways it has enriched our understanding of migration as an international phenomenon.⁴⁴

Stressing as it does the back-and-forth flow of people and information between the United States and Europe, the internationalist perspective reinforces the emphasis on cultural continuity that figures so prominently in the critique of Handlinesque uprootedness. It likewise resonates with the ideological dimension of postsixties revisionism. Merely placing "immigration" to the United States within the larger context of transnational "migration" in itself reduces the nationalistic overtones of the earlier strictly American focus. The ideological element is more evident in transnational studies that reflect a Marxian orientation. This is strongest in European-based work, some of which, according to a leading practitioner, is animated by a vision of international working-class solidarity.⁴⁵ Yet a generally New Left outlook is also discernible in the emphasis American scholars place on class and economic issues. In Bodnar's synthesis, for example, "capitalism" explains virtually everything; indeed, he originally planned to call his book "Children of Capitalism" rather than *The Transplanted*.⁴⁶ Still another connection with ideology has to do with "American exceptionalism," a term deriving from the Marxist tradition which scholars influenced by the New Left employ pejoratively, especially in reference to the "consensus" historians' emphasis on American uniqueness. The expression has not figured prominently in immigration

historiography, but one of the purposes of Walter Nugent's *Crossings* (1992) was to test the exceptionalist hypothesis by comparing European immigration to the United States with the same phenomenon in Argentina, Brazil, and Canada.⁴⁷

Not enough study has been devoted to multiculturalism as an object of analysis to permit confident generalization about it.⁴⁸ My own impression is that virtually all historians of immigration would accept it in the broadest (and weakest) sense, that is, as meaning that American society contains within itself a multitude of subgroups with distinctive cultural features, whether defined by race, ethnicity, religion, gender, language, or something else. But they have not played a prominent role in theorizing about it, especially by comparison to writers on education, literature, and the arts. More robust versions of multiculturalism, which tend to portray "America" as *nothing but* a collocation of culturally autonomous groups, perhaps exert greater appeal among historians who derive from ethnic groups officially designated as "minorities." Rodolfo Acuña, Ronald Takaki, and Gary Okihiro, for example, emphasize the racist oppressiveness of "Euro-Americans" and other themes characteristic of strong multiculturalism.⁴⁹ Historical work on women in immigration is still generally traditional in approach, but the powerful theoretical bent of women's studies could well move it too toward the stronger versions of multiculturalism.⁵⁰

Two features of recent work hark back to the prehistory of immigration historiography. The first is that today social scientists dominate the study of the immigration that is actually taking place just as they did in the opening decades of the century. Even the new historical work on Hispanics and Asians concentrates on the earlier phases of those groups' experience. Only David Reimers, Reed Ueda, and Elliott Barkan have written general historical accounts of the most recent immigration, and the leading book on the refugee issue since 1945 was co-authored by a political scientist and a lawyer.⁵¹

That historians should prefer a longer time-perspective, while social scientists devote themselves to contemporary phenomena, is a finding unlikely to generate much controversy. The second resemblance to the prehistory era will perhaps seem less bland, for it concerns the filiopietistic strain to be found in not a little of the newer work. As noted earlier, Edward Saveth—who put "filiopietism" into circulation and loaded it with pejorative connotations—had no patience with in-group historians who demanded "just recognition" for this or that immigrant people, claiming for them equal standing with "Anglo-Saxons in the soil of American nationality." Nor could he approve the notion that American history "should be re-written along 'racial or ethnic' lines"; on the contrary, ethnic cultures were to be understood, not as "separate entities," but as "aspects of a larger whole." Saveth also complained that filiopietistic accounts glorified outstanding individuals but paid little attention to ordinary folk.⁵² That's about the only part of his critique today's revisionists would agree with, for much of their own work—especially in its polemical and programmatic forms—is shot through with moral indignation at the prior historiographic neglect and social oppression suffered by whatever group is under consideration. Moreover, the demand for "just recognition" underlies the whole ethnic studies movement, and the goal of rewriting

American history with greater attention to racial and ethnic groups—and women—has guided the textbook industry since the late 1960s.

Because filiopianism has been academically disreputable for fifty years, no historian of immigration is likely to espouse it.⁵³ Indeed, one occasionally runs across a disclaimer to the effect that something just said, or about to be said, is “not filiopianistic”—which betrays a certain uneasiness about the applicability of the term. But perhaps it is time to reexamine the whole issue, which is at bottom a subspecies of the larger issue of historical objectivity. For it was, after all, professional historians’ putative commitment to objectivity that crucially distinguished them from “amateurs” and allowed them to avoid filiopianistic distortions. And in Saveth’s mind at least, professionalism entailed the assumption that American history had a master narrative, a national history to which the histories of the various ethnic groups must be related. These positions are no longer tenable as unanalyzed assumptions. The status of the objectivity question is very different now from what it was when Saveth wrote, to say nothing of what it was in the period of classical filiopianism; and many multiculturalists deny the existence of anything real to which a master narrative could refer. This is obviously not the place to undertake a rethinking of filiopianism. But it is perhaps apposite to note that historians must confront the existence of the filiopianistic strain in recent work before any such project can be carried out.

CONCLUSION

What has been concluded that we can draw conclusions about it? That rhetorical question—once addressed, I believe, to William James—fits our situation admirably, for everything touched on in this survey is still in process. History offers no more clear-cut lessons here than in most other areas. Our survey does, however, highlight recurrent themes that might repay further investigation.

One such theme is the relationship between changes in the overall intellectual climate and interpretive shifts in immigration history. Congruence between the two stands out most vividly in the 1960s, but it is discernible over the whole century under review. The influence of war as a factor in these concurrent changes might also be noted—most obviously in connection with World War II and Vietnam, but also in lesser degree in the case of World War I. Another kind of recurrent connection has to do with the ethnic composition of the historical profession and the attention devoted to immigration by different cohorts of historians. Here we find a positive correlation between what might be called the ethnic democratization of the profession and the degree and range of interest in ethnic subjects—with the present and recent past being most inclusive in both senses.

Another theme has to do with the relationships that exist between immigration historiography and the way American identity is understood. Conclusions here cannot help being impressionistic, but it seems clear that historians of different eras understood the relationship differently. Saveth established that the earliest

professional historians regarded immigration as incidental to national development. His book also suggests that their understanding of American identity was more ethnic than ideological, in the sense that it was shaped by the prevailing racialism of the times. It was also ethnic in the sense that it reflected their own "old American" derivation, which probably disposed them to agree with their fellow citizens of like background that unrestricted immigration could not continue indefinitely without imperiling the fabric of American society.

The first generation of professionals to make immigration history their specialty were of different ethnic background, and immigration was no longer a live issue politically when they wrote. Unlike their predecessors, they believed immigration was a significant historical subject; they were interested in the immigrant story for its own sake, and they stressed the contributions immigrants made to national development. But they, too, were assimilationists who accepted the view that in becoming Americans the immigrants were becoming part of a nation which had an identity of its own compounded of the various elements that went into it as they developed together over time. The terminology of race was still employed in discussions of immigration, but it was less determinative than formerly, and immigrant groups were more apt to be referred to as "nationalities." Conceptual issues of this sort were not, however, discussed systematically by historians.

By the Handlin era, immigration had become an even more strictly "historical" topic. Assimilation, it was thought, had all but completed its work so far as immigrant "nationality" was concerned, although religious differences remained and racial boundaries were even more resistant to change. Moreover, alienation and the mass society problem rendered assimilation a culturally ambiguous process. The penetration of social scientific thought made immigration historiography more conceptually sophisticated than before, one result of which was the discrediting of race, an effect massively reinforced by the counterexample of Nazism. The kind of group consciousness we now think of as "ethnic" was likewise discredited, for Nazism illustrated the grotesque extremes to which such *völkisch* thinking could be carried. For these reasons, and because national ideals were so strongly stressed in the war years, ethnicity was quite recessive and ideology quite dominant in the way midcentury historians of immigration thought of American identity.

That situation has been reversed in the most recent epoch. Immigration has returned with a vengeance, both as social reality and public issue. The ethnic and racial dimensions of group identity are more prominently featured than at any time since before World War I. Particularism enjoys a premium intellectually; universalism is at a severe discount. The ideological understanding of American national identity, though not driven completely from the field, is regarded by most as naive or even hypocritical. But those who champion some version of the ethnic perspective do not reject the basic values that constitute the core of the American ideology; on the contrary, they honor freedom, equality, and the dignity of the individual (although their emphasis on the group makes the latter

problematic). Their claim is that Americanism as traditionally understood failed to embody these values effectively—or, in some versions, was never intended to embody them.

Assuming that what has just been said about the presently dominant view is correct, it follows that the ideological element is still a vital constituent in the understanding of American identity, although it is much obscured by clamorous ethnic claims and recriminations about oppression. Contestation is the order of the day. From it, we may hope, there will eventually emerge a deeper and more satisfactory common understanding of what it means to be an American.

NOTES

1. John Higham, "Current Trends in the Study of Ethnicity in the United States," *Journal of American Ethnic History* [hereafter JAEH] 2 (fall 1982): 6–7; J. Hector St. John de Crèvecoeur, *Letters from an American Farmer* (New York, 1957), chap. 3. For discussion, see Moses Rischin, "Creating Crèvecoeur's 'New Man': He Had a Dream," JAEH 1 (Fall 1981): 26–42. Gary Gerstle, "Liberty, Coercion, and the Making of Americans," *Journal of American History* [hereafter JAH] 84 (September 1997): 524–58, which offers a different interpretation of immigration historiography, appeared too late to be considered here.

2. A. William Hoglund, *Immigrants and Their Children in the United States: A Bibliography of Doctoral Dissertations, 1885–1982* (New York, 1986), viii–ix; Edward N. Saveth, *American Historians and European Immigrants, 1875–1925* (New York, 1948), 9.

3. Saveth, *Historians and Immigrants*, 202–15.

4. Turner's articles appeared in the *Chicago Record-Herald*, August 28, September 4, 18, and 25, and October 9 and 16, 1901. His comments, especially in the fourth and sixth of these articles, reveal that Turner was troubled by the coming of the "new immigrants" around the turn of the century. For John R. Commons, see his *Races and Immigrants in America* (New York, 1907); for Zangwill and Kallen, see Arthur Mann, *The One and the Many: Reflections on the American Identity* (Chicago, 1979), chaps. 5–6.

5. Edith Abbott, *Immigration: Select Documents and Case Records* (Chicago, 1924); Abbott, *Historical Aspects of the Immigration Problem: Select Documents* (Chicago, 1926); Emily Greene Balch, *Our Slavic Fellow Citizens* (New York, 1910); Robert F. Foerster, *The Italian Emigration of Our Time* (Cambridge, Mass., 1919); W. I. Thomas and Florian Znaniecki, *The Polish Peasant in Europe and America*, 5 vols. (Boston, 1918–20); Arthur M. Schlesinger, *New Viewpoints in American History* (New York, 1922).

6. Schlesinger, *New Viewpoints*, 16; for Turner, see Saveth, *Historians and Immigrants*, 122–37; Theodore C. Blegen, ed., *Land of Their Choice: The Immigrants Write Home* (Minneapolis, 1955), ix–x; Moses Rischin, "Marcus Lee Hansen: America's First Transethnic Historian," in Richard L. Bushman et al., *Uprooted Americans: Essays to Honor Oscar Handlin* (Boston, 1979), 333–38; and Frederick C. Luebke, "Turnerism, Social History, and the Historiography of European Ethnic Groups in the United States," in Luebke, *Germans in the New World* (Urbana and Chicago, 1990), 138–56.

7. George M. Stephenson, *Political History of the Public Lands* (Boston, 1917); Stephenson, *A History of American Immigration, 1820–1924* (Boston, 1926), 3, and chaps. 16–17.

8. *Ibid.*, 264.
9. For discussion of this group, see O. Fritiof Ander, "Four Historians of Immigration," in Ander, ed., *In the Trek of the Immigrants: Essays Presented to Carl Wittke* (Rock Island, Ill., 1964), 17–32.
10. William Forbes Adams, *Ireland and Irish Emigration to the New World from 1815 to the Famine* (New Haven, 1932); Ray Allen Billington, *The Protestant Crusade: A Study of the Origins of American Nativism* (New York, 1938).
11. See Philip Gleason, *Speaking of Diversity: Language and Ethnicity in Twentieth-Century America* (Baltimore, 1992), chaps. 6–7, and the literature cited there.
12. George M. Stephenson, *The Religious Aspects of Swedish Immigration* (Minneapolis, 1932); Theodore C. Blegen, *Norwegian Migration to America*, 2 vols. (Northfield, Minn., 1931–40). See also John Higham, "The Ethnic Historical Society in Changing Times." *JAEH* 13 (winter 1994): 30–44.
13. Carl Wittke, *We Who Built America: The Saga of the Immigrant* (New York, 1939), 518, v. In the last-quoted passage, Wittke was probably alluding to, and implying the incompleteness of, James Truslow Adams's widely read book, *The Epic of America* (Boston, 1931).
14. Wittke, *We Who Built*, xviii.
15. This review (*American Historical Review* 37 [April 1932]: 572–73), which also covered a book on Swedish immigration by Florence E. Janson, did not endear Hansen to Blegen's friends. It still rankled with Carlton Qualey more than thirty years later; others expressed skepticism that Hansen had full mastery of the Scandinavian languages. See Qualey, "Marcus Lee Hansen," *Midcontinent American Studies Journal* 8 (fall 1967): 18–25; and Rischin, "Hansen," 328–29.
16. Marcus L. Hansen, *The Atlantic Migration, 1607–1860* (Cambridge, Mass., 1940); for discussion, see Rischin, "Hansen," 319–47, and Moses Rischin, "Just Call Me John: Ethnicity as *Mentalité*," in Peter Kivisto and Dag Blanck, eds., *American Immigrants and Their Generations* (Urbana and Chicago, 1990), 64–82.
17. For discussion of the relationship between immigrants and members of the founding "charter group," see John Higham, *Send These to Me: Jews and Other Immigrants in Urban America* (New York, 1975), chap. 1.
18. Marcus L. Hansen, *The Immigrant in American History* (Cambridge, Mass., 1940); the other two essays, "The Problem of the Third-Generation Immigrant," and "Who Shall Inherit America?" are both reprinted in Kivisto and Blanck, eds., *Immigrants and Their Generations*, 191–203, 204–13.
19. *Ibid.*, 201–2, 210, 198–201.
20. See Rischin, "Just Call Me John," 69–71, 76–77.
21. Oscar Handlin, *Boston's Immigrants: A Study in Acculturation* (Cambridge, Mass., 1941); Handlin, *The Uprooted: The Epic Story of the Great Migrations That Made the American People* (Boston, 1951).
22. Rudolph J. Vecoli, "Contadini in Chicago: A Critique of *The Uprooted*," *JAH* 51 (December 1964): 407–17. Bushman et al., *Uprooted Americans*, includes a sketch of Handlin's career, a bibliography of his writings, and a listing of the doctoral students he directed up to the date of publication (1979); see also Maldwyn Allen Jones, "Oscar Handlin," in Marcus Cunliffe and Robin W. Winks, eds., *Pastmasters: Some Essays on American Historians* (New York, 1969), 239–77; David J. Rothman, "The Uprooted: Thirty Years Later," *Reviews in American History* 10 (September 1982): 311–19; and Reed Ueda, "Immigration and the Moral Criticism of History: The Vision of Oscar Handlin," *Canadian Review of American Studies* 21 (fall 1990): 183–201.

23. Fuller development of, and documentation for, the interpretation advanced in this and following paragraphs may be found in Gleason, *Speaking of Diversity*, chaps. 3–9.

24. It is revealing that the main title, *One America*, was added to the 1946 and 1952 editions of the minencyclopedia of American minorities edited by Francis J. Brown and Joseph S. Rousek, which appeared before the war under the title *Our Racial and National Minorities* (New York, 1937).

25. Handlin, *Uprooted*, 3 (quotations are from the Grosset and Dunlap paperback edition [New York, n.d.]).

26. *Ibid.*, 4, 6.

27. *Ibid.*, 4.

28. *Ibid.*, 304–5.

29. John Higham, *Strangers in the Land: Patterns of American Nativism, 1860–1925* (New Brunswick, N.J., 1955); Maldwyn Allen Jones, *American Immigration* (Chicago, 1960).

30. The following essays, all by Rudolph J. Vecoli, provide an excellent guide to the explosion of research: “Ethnicity: A Neglected Dimension of American History,” in Herbert J. Bass, ed., *The State of American History* (Chicago, 1970), 70–88; “European Americans: From Immigrants to Ethnic,” in W. H. Cartwright and R. L. Watson, eds., *The Reinterpretation of American History and Culture* (Washington, D.C., 1973), 81–112; “The Resurgence of American Immigration History,” *American Studies International* 17 (Winter 1979), 46–66; “Return to the Melting Pot: Ethnicity in the Eighties,” *JAETH* 5 (fall 1985): 7–20; “From *The Uprooted* to *The Transplanted*: The Writing of American Immigration History 1951–1989,” in V. G. Lerda, ed., *From ‘Melting Pot’ to Multiculturalism* (Rome, 1990), 25–53; “An Inter-Ethnic Perspective on American Immigration History,” *Mid-America* 75 (April–July 1993): 223–35. For Vecoli’s more recent substantive contributions, see “Ethnicity and Immigration” in Stanley Kutler, ed., *Encyclopedia of the United States in the Twentieth Century*, vol. 1 (New York, 1996), 161–93; and “The Significance of Immigration in the Formation of an American Identity,” *The History Teacher* 30 (November 1996): 9–17.

31. Høglund, *Immigrants and Their Children*, xxiii. Approximately 30 percent of the dissertations were in history in the 1950s; approximately 20 percent in the 1970s. Immigration-related dissertations in education and English increased the most in those two decades.

32. For more on the interpretation advanced here, see my essay “American Identity and Americanization,” in Stephan Thernstrom, Ann Orlov, and Oscar Handlin, eds., *Harvard Encyclopedia of American Ethnic Groups* (Cambridge, Mass., 1980), 52–55.

33. Statistics derived from U.S. Bureau of the Census, *Statistical Abstract of the United States: 1993* (Washington, D.C., 1993), 11, table 8. For discussion, see David Reimers, *Still the Golden Door: The Third World Comes to America*, 2nd ed. (New York, 1992); Reed Ueda, *Postwar Immigrant America* (Boston, 1994).

34. Carey McWilliams, *North From Mexico*, rev. ed. (New York, 1990), esp. chaps. 17–18, written by Matt S. Meier; David R. Colburn and George E. Pozzetta, “Race, Ethnicity, and the Evolution of Political Legitimacy,” in David Farber, ed., *The Sixties: From Memory to History* (Chapel Hill, 1994), 119–48; Perry L. Weed, *The White Ethnic Movement and Ethnic Politics* (New York, 1973); Mann, *One and Many*, chaps. 1–2.

35. Nathan Glazer and Daniel P. Moynihan, *Beyond the Melting Pot* (Cambridge, Mass., 1963); Milton M. Gordon, *Assimilation in American Life* (New York, 1964); for historiographical developments, see John Higham, *History* (Baltimore, 1989), 235–64; Higham, “The Future of American History,” *JAH* 80 (March 1994): 1289–1309; and Peter Novick, *That Noble Dream: The ‘Objectivity Question’ and the American Historical Profession* (Cambridge, 1988), part 4.

36. See Donna Gabaccia and James Grossman, "Introduction: The Teaching of Immigration History," in Gabaccia and Grossman, compilers, *Teaching the History of Immigration and Ethnicity: A Syllabus Exchange* (Chicago, mimeo., 1993).

37. David Hollinger, *Postethnic America: Beyond Multiculturalism* (New York, 1995), chap. 2, esp. 38–39; Werner Sollors, ed., *The Invention of Ethnicity* (New York, 1989); Kathleen N. Conzen et al., "The Invention of Ethnicity: A Perspective from the USA," *JAETH* 12 (fall 1992): 3–63; M. G. Smith, "Ethnicity and Ethnic Groups in America: The View from Harvard," *Ethnic and Racial Studies* 5 (1982): 1–22.

38. For Mexican-American efforts to be classified as a race, see Hollinger, *Postethnic America*, 33; for whiteness, David R. Roediger, *The Wages of Whiteness* (London, 1991), and Noel Ignatiev, *How the Irish Became White* (New York, 1994); for the development of affirmative action categories, Gleason, *Speaking of Diversity*, 102 ff., and the literature cited there; for operation of the policy, Lawrence H. Fuchs, *The American Kaleidoscope: Race, Ethnicity, and the Civic Culture* (Middletown, Conn., 1990), 381–457.

39. For a sampling of views on these matters, see Russell Nieli, ed., *Racial Preference and Racial Justice* (Washington, D.C., 1991); John Higham, "Multiculturalism and Universalism: A History and Critique [and comments thereon]," *American Quarterly* 45 (June 1993): 195–256; Harold K. Bush, Jr., "A Brief History of PC, with Annotated Bibliography," *American Studies International* 33 (April 1995): 42–64; and Nathan Glazer, *We Are All Multiculturalists Now* (Cambridge, Mass., 1997).

40. See Russell A. Kazal, "Revisiting Assimilation: The Rise, Fall, and Reappraisal of a Concept in American Ethnic History," *American Historical Review* 100 (April 1995): 437–71; U.S. Commission on Immigration Reform, *Becoming an American: Immigration and Immigrant Policy* (Washington, D.C., 1997); and Gleason, *Speaking of Diversity*, chap. 2.

41. Vecoli quoted from his "Introduction" to Rudolph J. Vecoli and Suzanne M. Sinke, eds., *A Century of European Migrations, 1830–1930* (Urbana and Chicago, 1991), 11; John Bodnar, *The Transplanted: A History of Immigrants in Urban America* (Bloomington, Ind., 1985); Jon Gjerde, *The Minds of the West: Ethnocultural Evolution in the Rural Middle West, 1830–1917* (Chapel Hill, 1997). For general discussion of the interpretive shift, see Peter Kivisto, "The Transplanted Then and Now: The Reorientation of Immigration Studies from the Chicago School to the New Social History," *Ethnic and Racial Studies* 13 (October 1990): 455–81; and Ewa Morawska, "The Sociology and Historiography of Immigration," in Virginia Yans-McLaughlin, ed., *Immigration Reconsidered* (New York, 1990), 187–238.

42. Nathan Glazer, *Affirmative Discrimination: Ethnic Inequality and Public Policy* (New York, 1975), 177–78.

43. Thistlethwaite's 1960 paper is reprinted, with interesting retrospective comments by the author, in Vecoli and Sinke, *Century of European Migrations*, which is the best introduction to this literature.

44. Josef J. Barton, *Peasants and Strangers* (Cambridge, Mass., 1975); Dirk Hoerder, ed., "Struggle a Hard Battle": *Essays on Working Class Immigrants* (De Kalb, Ill., 1986); Walter Nugent, *Crossings: The Great Transatlantic Migrations, 1870–1914* (Bloomington, Ind., 1992); Mark Wyman, *Round-Trip to America: The Immigrants Return to Europe, 1880–1930* (Ithaca, N.Y., 1993); H. Arnold Barton, *A Folk Divided: Homeland Swedes and Swedish Americans, 1840–1940* (Carbondale, Ill., 1994).

45. See, for example, Dirk Hoerder, *American Labor and Immigration History, 1877–1920s: Recent European Research* (Urbana and Chicago, 1983), 3–15.

46. For this point, see Kivisto, "Transplanted Then and Now," 472, 477; see also the comments on Bodnar's book by James R. Barrett and John J. Bukowczyk in *Social Science History* 12 (fall 1988): 221–31, 233–41.

47. Nugent, *Crossings*, 5–6, 164–65.

48. For a variety of perspectives, see Hollinger, *Postethnic America*; Higham, “Multiculturalism and Universalism”; Werner Sollors, “E Pluribus Unum; or Matthew Arnold Meets George Orwell in the ‘Multiculturalism Debate’” (working paper no. 53/1992 of the John F. Kennedy Institute for North American Studies, Freie Universität Berlin); the chapters by Reed Ueda (“Ethnic Diversity and National Identity in Public School Texts”) and Gary B. Nash (“American History Reconsidered: Asking New Questions about the Past”) in Diane Ravitch and Maris Vinovskis, eds., *Learning from the Past: What History Teaches Us about School Reform* (Baltimore, 1995), 113–34 (Ueda), 135–63 (Nash); and Avery F. Gordon and Christopher Newfield, eds., *Mapping Multiculturalism* (Minneapolis, 1996).

49. Rodolfo Acuña, *Occupied America: A History of Chicanos*, 3rd ed. (New York, 1988); Ronald T. Takaki, *A Different Mirror: A History of Multicultural America* (Boston, 1993); Gary Okihiko, *Margins and Mainstreams: Asians in American History and Culture* (Seattle, 1994).

50. For a recent synthesis, see Donna Gabaccia, *From the Other Side: Women, Gender, and Immigrant Life in the U.S., 1820–1990* (Bloomington, Ind., 1994); for a more theoretical emphasis, see Sydney Stahl Weinberg, “The Treatment of Women in Immigration History: A Call for Change,” in Donna Gabaccia, ed., *Seeking Common Ground: Multidisciplinary Studies of Immigrant Women in the United States* (Westport, Conn., 1992), 3–22.

51. Reimers, *Still the Golden Door*; Ueda, *Postwar Immigrant America*; Ueda, *The Permanently Unfinished Country* (forthcoming); Elliott Robert Barkan, *And Still They Come* (Wheeling, Ill., 1996); Gilbert D. Loescher and John A. Scanlan, *Calculated Kindness: Refugees and America’s Half-Open Door, 1945–Present* (New York, 1986). See also Francesco Cordasco, *The New American Immigration: Evolving Patterns of Legal and Illegal Emigration: A Bibliography of Selected References* (New York, 1987).

52. Saveth, *Historians and Immigrants*, 202–15.

53. Critical attention to filiopietism in recent works is extremely rare, but its presence is noted in Oscar Handlin, *The Uprooted*, 2nd ed., enlarged (Boston, 1973), 330, note 53; David Noel Doyle and Owen Dudley Edwards, eds., *America and Ireland, 1776–1976: The American Identity and the Irish Connection* (Westport, Conn., 1980), 313–14; Diane Ravitch, “Multiculturalism: E Pluribus Plures,” *American Scholar* 59 (summer 1990): 341, 346; and Ueda, “Ethnic Diversity and National Identity,” in Ravitch and Vinovskis, eds., *Learning from the Past*, 126.

The Relevance and Irrelevance of American Colonial History

GORDON S. WOOD

AMERICANS have always had a special problem relating to the colonial period of their history. Although nearly as many years separate the first European explorations of North America from the beginning of the United States as separate the beginning of the United States from the present, Americans have often thought the earlier colonial period to be less relevant, less historically significant than the later national period of American history. For many Americans the colonial era has lacked seriousness; it seems trivial and antique and shrouded in nostalgia. For much of American history popular opinion has considered the century and a half of the colonial period to be simply a quaint prologue to the main national story that followed the American Revolution.

In part this is because the colonial period has become the Americans' natural source of folklore and myth-making. Since Americans, unlike other Western nations, lack a misty past where the historical record is remote and obscure, they have tended to turn authentic historical figures and events of their colonial past into mythical characters and legends. In America there are no King Canutes, no King Arthurs, no Robin Hoods to spin tales and legends about. Instead, Americans have transformed John Smith and Pocahontas, the Pilgrim Fathers and Squanto—historical figures about whom we know a good deal—into fanciful and fabulous characters.¹ As a consequence of this kind of myth-making, time does not have the same meaning in the seventeenth and eighteenth centuries as it does in the nineteenth and twentieth centuries. Even modern professional historians, who presumably should know better, have tended to foreshorten and telescope the colonial period in strange ways. Twentieth-century textbook writers, for example, have squeezed into a single paragraph accounts of Bacon's Rebellion in 1676 and the uprising of the Paxton Boys in 1763–64 as incidents of colonial violence.² Of course, they would never think of doing the same thing in the national period: they would never lump together discussions of the Whiskey Rebellion in 1794 with the march of Coxey's Army on Washington in 1894 or place in the same paragraph the draft riots of the 1860s with the black riots of the 1960s. Apparently time in the colonial period is considered shorter and more compressible.

Events in the colonial period often seem less real and less significant than those in the national period. In school children are taught about Columbus and the

discovery of America, and then around Thanksgiving they learn about the Pilgrims and Plymouth Rock and the feast with the Indians at the end of the first difficult year in the New World. Two centuries are often collapsed into a few quasi-mythical events preceding the Revolution, when the real history of the United States presumably begins. Thus it is not surprising that the colonial period has often appeared remote and detached from the rest of American history. As the repository of America's myths and legends and the source for schoolchildren's sentimental stories, the colonial period has often seemed to modern Americans to be irrelevant and unconnected to the national history of the United States and not an object of serious historical study.

But this has not always been the case. In the decades following the Revolution the colonial period was very much an integral and important part of American history. The Revolutionary leaders took the colonial period seriously indeed. They tended to look back to the seventeenth-century settlements, in John Adams's words, "as the opening of a grand scene and design in Providence for the illumination of the ignorant and the emancipation of the slavish part of mankind all over the earth."³ In such Revolutionary sentiments lay the sources for the emerging notion of America as an exemplary nation. The United States, the revolutionaries declared, was a new kind of nation and the best hope for saving the world from corruption and tyranny.⁴ But America's special role in the world could not be appreciated properly by focusing exclusively on the Revolution. Full understanding of America and its unique place in the world required going back to the original settlements of the seventeenth century or earlier. Right from the beginning of the United States historians and fiction writers began using the colonial period to work out problems of national identity.⁵ For the citizens of the early republic America's colonial origins thus could not be simply a source of folklore and romantic legends; the colonial era was essential to an understanding of the whole progressive story of the United States. The founding of the nation lay not with the Declaration of Independence in 1776 but with the early explorations or, more often, with the earliest settlements and events of the seventeenth century—with Jamestown in 1607, John Winthrop and the Puritans in 1630, and Lord Baltimore's statute of religious toleration in 1649.

New Englanders of the early republic in particular celebrated the achievements of the Massachusetts Puritans in creating the sense of America as "a city upon a hill," as an exemplar for the world and as an asylum for religious liberty. In the 1820s citizens of the early republic reprinted numerous seventeenth-century Puritan texts, including Cotton Mather's *Magnalia Christi Americana* in 1820, John Winthrop's *History of New England* in 1825, and Nathaniel Morton's *New England's Memorial* in 1826.⁶ But important as the Puritans as a special godly people were in the founding of the nation, their story could not compare in poignancy with that of the simple Pilgrims of Plymouth Colony, as told by their leader William Bradford. This was the story of a small band of English refugees, numbering only a hundred or so, driven from their homes for their religious views, journeying first to Holland and then to the New World, binding themselves together with their "Mayflower Compact" in 1620 in an apparently democratic

fashion, suffering terrible losses their first year in Plymouth, and all along wanting nothing more than to be left alone to practice their "Separatist" religion. As one-time New Hampshire congressman Salma Hale pointed out in his *History of the United States* (1826), the experience of these Pilgrim immigrants offered a lesson to the new republic on how to build "a body politic, for the purpose of making equal laws for the general good."⁷ In celebrating the special role of the Pilgrims and Puritans in the messianic founding of the United States, New Englanders and other Americans of the early nineteenth century never lost the sense that these seventeenth-century stories were integrally related to the subsequent history of the country. Indeed, wrote David Ramsay in his posthumously published *History of the United States* (1818), the seventeenth-century New England Puritans "were advanced a century a-head of their contemporaries, in the school of republicanism and the rights of man."⁸

In his 1802 commemoration of the landing of the Pilgrims on Plymouth rock John Quincy Adams tried to expand the number of founders, and he listed Sir Walter Raleigh, John Smith, George Calvert, William Penn, and George Oglethorpe as men who "excite in our minds recollections equally pleasing, and gratitude equally fervent" with those of Bradford and Winthrop.⁹ Although orators and writers like Adams and others described these seventeenth-century founders in heroic and filio pietistic terms, they nevertheless did not mythologize them but treated them as authentic and significant historical figures whose stories necessarily made up the first stage in the development of the later United States.

Historians in the early years of the nation's existence had little doubt of the relevance of the colonial period to the story of America. Professor Samuel Williams of Harvard writing his *History of Vermont* in 1794 had no doubt that the foundations of American freedom were laid in the social developments of the colonial era. Jeremiah Belknap believed that he had to begin at the very beginning of exploration for his biographical dictionary of important individuals in American history. He thus devoted two volumes to the original explorers and settlers, ranging from the Phoenicians and Columbus to George Calvert and William Penn; he died in 1798 before he could get out of the seventeenth century. John Marshall likewise thought that American history necessarily began with the earliest colonial settlements. He therefore spent the entire first volume of his five-volume biography of George Washington (1804–7) describing the history of the colonies before he even got to Washington's birth.¹⁰

It is not surprising then that when George Bancroft, the young nation's first significant historian, sought in the 1830s to tell the complete story of America he would go back to its early colonial beginnings. Bancroft declared that he "dwelt at considerable length" on the seventeenth century in his great history of America "because it contains the germ of our institutions."¹¹ He began his history with the Icelandic voyages and Columbus's discovery and intended to bring it up to his own time, but in the end he carried his ten-volume history only through the peace with England in 1783. (Only later did he add the formation of the Constitution.) For Bancroft and his fellow Americans the colonial period was the natural source of the whole of American history. It was the nation's youth. It

flowed naturally into the national history that followed and was intimately connected with it. After all, "the maturity of the nation is but a continuation of its youth." Of course, that youth did not know all that the mature nation knew. The early colonists did not know about religious freedom and democracy as people of the nineteenth century did, but the seeds of these developments were planted in the colonial period. The American people had gradually learned about these principles and had progressed and developed into a more free and democratic nation than any people before them had known. More than any other work, Bancroft's *History of the United States* contributed to the belief that the American republic was showing the world the way toward true liberty and democracy.¹²

Bancroft's view of the colonial past was highly whiggish; he saw the past as simply an anticipation of the present and future. In volume one of his history of the nation, even in the fifteenth edition, he entitled the chapter describing the early English voyages to North America, including the Roanoke settlement in the 1580s, as "England takes possession of the United States."¹³ The colonies were already the nation in embryo; the United States and the spirit of freedom were present from the earliest beginnings of exploration and settlement. Since history for Bancroft was just the realization of these earliest beginnings, his readers could have no doubt of the relevance of colonial history: it had to be an essential part of the history of the United States, and all Americans had a stake in it.

Yet this Bancroftian conception of the colonial past as an integral part of the whole of American history did not last. Americans of the later nineteenth century did not continue to regard the colonial period as relevant and essential to the rest of American history as they had at the beginning of the century. Even as early as the middle decades of the nineteenth century many Americans began viewing the colonial period in very different ways and gradually separating it from the rest of American history. The sectional conflict that led to the Civil War resulted in the increasing divergence of the South from the mainstream of American history.

In his *History of the United States* Bancroft had labored to portray seventeenth-century Virginia's being as devoted to liberty and the Parliamentary cause in the English Civil War as Puritan Massachusetts had been. But some southern critics balked at Bancroft's interpretation. As early as 1835 southerners rejected what they called Bancroft's "strange attempt to pervert the truth of history" by identifying Virginia's history with that of Massachusetts; and they refused to "acquiesce in the new notion 'that the people of the colonies, all together, formed one body politic before the Revolution.'" Instead of emphasizing its common colonial beginnings with the rest of the United States, the South now began claiming its special aristocratic origins. The southern colonies, especially Virginia, it was said, had been settled by royalist Cavaliers, that is, by supporters of Charles I in England. In contrast, the northern colonies had been settled by Roundhead Puritans, by narrow-minded plebeian people who had no aristocratic taste or grace. That may have been true, retorted some New England writers, but at least New Englanders were the ones who had "the exclusive honour of having *originated* the free principles" that had come to characterize the United States. At the very moment the Virginians were "importing into the country a cargo of negroes, to entail

the curse of slavery on their remotest posterity, . . . our first fathers were founding the liberties of America on the Plymouth rock."¹⁴

These different sectional conceptions of the colonies' origins contributed to the Cavalier myth of the South and helped to justify what seemed by the 1840s and '50s to be the very distinctive aristocratic southern culture.¹⁵ A nation that was coming apart could no longer have a colonial past that belonged equally and uniformly to all parts of it. As the sectional crisis deepened, Bancroft's view that the colonial period was essential to the subsequent democratic story of the United States lost much of its significance.

There were other reasons too for the repudiation of Bancroft's interpretation and the gradual separation of the colonial era from the rest of American history. During these same mid-nineteenth-century decades patrician elites in the North began looking to their colonial roots as a means of distinguishing themselves from the common people who were gaining increasing authority in democratic America, and in the process they gradually cut many of the ties that had hitherto bound the colonial past to the national history of the country. Through genealogical investigations they sought to assert their pride of ancestry and their special relationship to the colonial period. In 1844 the New England Historic-Genealogical Society was founded, followed in 1869 by a similar organization in New York. Everywhere in the Northeast state and local historical societies tended to become centers for genealogical and antiquarian interest. In the decades following the Civil War, these tendencies were accentuated. As the United States became more urbanized and industrialized, with ever increasing numbers of new non-Anglo-Saxon and non-Protestant immigrants, many old-stock Yankees organized a variety of societies designed to establish the priority of their ancestors as Americans. These included the Sons of the American Revolution (1889), the Daughters of the American Revolution (1890), the Colonial Dames of America (1890), the Society of Mayflower Descendants (1897), and many others. Members of these societies claimed that they had a special connection to the colonial past, that it was peculiarly their preserve and not, as it had been earlier, an integrated part of the rest of American history.¹⁶

At first the New England Historic-Genealogical Society had been concerned only with those ancestors who had come to America before 1700. But perhaps because of the need to expand the subscription rolls, this date was eventually moved forward to 1776. By the late nineteenth century many members of these various genealogical and antiquarian societies were claiming that any ancestor who settled in America before the Declaration of Independence should be included among the nation's founders.¹⁷ They implied that those citizens whose ancestors came to the New World after 1776 were something less than full Americans.

Various ethnic groups responded to these implications by establishing their own patriotic societies in order to throw out their own lifelines to the colonial past and to document their particular ancestors' contributions to America's heritage. Thus was organized the Huguenot Society (1883), the Holland Society (1885), the Scotch-Irish Historical Society (1889), the American Jewish Histori-

cal Society (1892), the American-Irish Historical Society (1897), and the German-American Historical Society (1897).¹⁸ All these ethnic groups sought, sometimes desperately, to establish some sort of historical relationship with the colonial era.

Despite these efforts, however, many Americans increasingly regarded the colonial period as something detached and separate from the rest of American history, as something that did not belong equally to all Americans. It was at the time of the centennial celebrations of the Revolution and especially of the formation of the Constitution that the designation of “founding fathers” was shifted from the seventeenth-century settlers to the Revolutionary leaders of 1776 and the Constitution-makers of 1787. Influential members of the Massachusetts Historical Society began calling for less attention to be paid to colonial origins and more to the national period of American history. What had gone on before the Revolution was no longer as important to the history of the United States as it once had been.¹⁹

Developments in American popular culture contributed to that view. During the latter part of the nineteenth century the colonial past became less and less a period of authentic historical actors and events and more and more a nostalgic repository of an imagined and mythic America—a simple bucolic world that was free of the sprawling slums and ethnic diversity of a modern urban society. Instead of seeing, as Bancroft had, the colonial era as containing the seeds of later American democracy and being a continuous and integral part of the whole story of the republic, many late nineteenth-century Americans saw it as unrelated to subsequent American history, indeed, as a point of contrast to the more complicated and sordid world that had followed. Painters like Francis David Millet and writers like Alice Morse Earle portrayed clean and cosy colonial scenes that appealed deeply to the longings of late nineteenth-century Americans. In 1896 Henry Wadsworth Longfellow’s poetic depiction of seventeenth-century Plymouth, *The Courtship of Miles Standish*, was republished and built into the curriculums of many of the nation’s schools.

The colonial period seemed more and more to belong to American folklore and the story-telling of elementary school, and not to serious historical study. Everywhere colonial antiques and old-country furnishings told Americans of a lost past. The “clear white houses” of an old New England village were to a character in Henry James’s novel *Roderick Hudson* (1875) representations of “kindness, comfort, safety, the warning voice of duty, the perfect absence of temptation.” Much of the colonial revival movement in art, architecture, and literature that took place at the end of the nineteenth century and the beginning decades of the twentieth was designed to Americanize the new immigrants and teach them what the first curator of the American Wing of the Metropolitan Museum of Art in 1925 called the ideals and values “held by the men who gave us the Republic.”²⁰ Yet the colonial revival movement had the effect of sentimentalizing the colonial period and making it seem irrelevant to the lives of the new and recent immigrants. These developments reached their peak during the first four decades of the twentieth century, expressed most remarkably perhaps by the restoration of

Colonial Williamsburg in the 1930s and '40s. As this popular and sentimental interest in the colonial period grew, however, not only was the period further separated from the rest of American history, but professional historical interest in it gradually declined.

At the time of the founding of the *American Historical Review* in 1895 and the beginnings of the American historical profession the colonial period still dominated the writing of American history just as it had for Bancroft's generation. Not only did the amateur antiquarians centered in the state and local historical and genealogical societies continue to focus on the colonial sources of America, but Herbert Baxter Adams, often considered, perhaps mistakenly, to be the first of American professional historians, devoted most of his energies in the 1870s and '80s to promoting the study of local institutions in the colonial period, including the New England town. Adams and many of his students were obsessed with continuities between Europe and America, with what has been called a "germ theory," and they reached back for the origins of America's local institutions not just to the colonial period but all the way back to Anglo-Saxon England and the Teutonic forests of Germany.²¹

The new professional historians quickly showed that the Teutonic germ thesis was improbable and undemonstrable and already undermined by English scholars. J. Franklin Jameson, the first Ph.D. under Adams at Johns Hopkins University, continually moaned over the unscientific character of the local studies, especially over having to trace institutions "back *nearly* to when our ancestors chattered in the tree tops."²² But these complaints were nothing compared with the scorn the new professionals directed at the parochialism and mindless fact-collecting of most colonial scholarship. They were desperate to escape from what Jameson sneeringly called "the local and antiquarian details of the colonial period."²³ Some of their frustration with the triviality of colonial scholarship was captured by a young Hopkins graduate student, Woodrow Wilson, who complained in 1884 of going into his colonial history examination "crammed with one or two hundred dates and one or two thousand minute particulars about the quarrels of nobody knows who with an obscure Governor, for nobody knows what. Just think of all that energy wasted! The only comfort is that this mass of information won't long burden me. I shall forget it with great ease."²⁴

It was not that the new professionals wished to ignore the colonial period; quite the contrary. But they wanted a much more cosmopolitan and more professional perspective brought to bear on it. They aimed to study the colonial period seriously and scientifically. They were disgusted with the way colonial America had been romanticized and littered with myths and false legends. They were determined to clean up this mythical litter and to set straight the history of the colonial past. Ironically, however, these efforts by the new professional historians to write a more scientific history of early America in the end only contributed further to detaching the colonial period from the subsequent national history of the United States.

At the beginning of the twentieth century there were three major emerging schools of professional historians concerned with early America—the imperial

historians, the Progressive historians, and those historians connected with Frederick Jackson Turner and his frontier thesis. Although each of these schools began by studying early America, each of them inadvertently helped to set the colonial period apart from the rest of American history. Each of them suggested in different ways that the colonial era was not naturally connected with the national history of the United States, and each implied that the colonial period had not fundamentally contributed to the making of what was distinctive or unique about American institutions and culture. The work of these several schools of professional historians eventually undermined all that Bancroft and his generation had believed about the continuity and relevance of the colonial period to the subsequent history of America.

The so-called imperial school, led by Herbert Osgood and Charles McLean Andrews, thought that nineteenth-century historians had been much too parochial and antiquarian in their attitude toward early American history. The nineteenth-century historians had too often viewed the "colonial era as a tangled mass of genealogical tree roots." They had focused too narrowly on the states and localities and had been unable to distinguish between "the really important and the insignificant." Previous historians had concentrated on each of the thirteen colonies at the expense of the whole and had ignored the other British colonies in the Western Hemisphere. They had enveloped "men and events connected with our colonial past in an atmosphere of piety, patriotism, and perfection." Both Osgood and Andrews, as good professionals, wanted colonial history to escape from its previous triviality and parochialism and become more impartial and cosmopolitan. Early American history, they said, ought to be looked at "not only from the colonial but from the British standpoint."²⁵

The result was a half century or more of important studies of the American colonies that tended to view them from the vantage point of London. The history of the thirteen continental colonies became less the history of the origins of the United States and more the history of some of the outposts of the expanding first British empire. In fact, the imperial scholars argued that their history should not confine itself to the thirteen colonies that became the United States but should embrace all the western hemispheric colonies of the British empire, the West Indian and Canadian as well as the mainland colonies. The breadth and depth of this imperial scholarship was truly remarkable. However rich and significant, however, it had the ultimate effect of separating the colonial period from the rest of American history. In the hands of the imperial historians the history of the colonies became largely a branch of British history, not the first stage in the development of the American nation. As Andrews put it, "The years from 1607 to 1783 were colonial before they were American or national, and our Revolution is a colonial and not an American problem."²⁶

The work of Frederick Jackson Turner and his followers, writing more or less at the same time during the first half of the twentieth century, accentuated this detachment of the colonial period from the national history of the United States. Unlike the imperial historians, Turner was exclusively and unequivocally interested in the roots of Americanism. Indeed, probably no historian has contributed

more to the myth of American exceptionalism. Above all, Turner sought to explain what he saw as the peculiarities of the American character—the democratic and individualistic tendencies of the American people. He saw the source of that character in the availability of free virgin land. The West for Turner was not important for its own sake. Rather, he said, “the distinctive thing about the West is its relation to free lands; and it is the influence of her free lands that has determined the larger lines of American development” and has set “the evolution of American and European institutions in contrast.”

Turner saw three phases of American growth. The first ran from the first European settlements to the time of the Revolution. The second, the golden age of frontier history, went from the Revolution to 1890. The third, in which Turner now lived, saw the end of free lands and the beginnings of an entirely new phase of American history. “With conditions comparable to those of Europe,” he wrote in 1895, “we have to reshape the ideals and institutions fashioned in the age of wilderness-winning to the new conditions of an occupied country.”

In Turner’s opinion the first phase of the colonial period—“the application of European men, institutions, and ideas to the tide-water area of America”—did not have much influence on the peculiar development of America. Although some modification of European culture took place during this colonial period, “English traits and institutions preponderated.” During most of the first century and a half of colonial history the colonists remained confined to a several-hundred-mile or so strip of the eastern coast of North America. “The constant touch of this part of the country with the Old World prevented the modifying influences of the new environment from having their full effect, and the coast area seemed likely to produce institutions and men that were but modified shoots from the parent tree.” Even the physical features of the colonists, he said, were still English: the colonists remained ruddy in appearance and without the nervous energy or expressiveness of later Americans. During the first phase of American development—the colonial period—the settlers, according to Turner, remained still essentially Europeans.

Only during the second phase of American growth, at the end of the eighteenth and the beginning of the nineteenth centuries, did the “process of Americanization” really take place. This phase began after the Seven Years’ War “with the spread of this colonial society towards the mountains; the crossing of the Alleghenies, and the settlement upon the Western Waters. . . . As each new advance occurred, the process was repeated with modifications. In this reaction between the West and the East, American society took on its peculiar features.” Thus only when Americans had broken free of the British imperial shackles and entered the trans-Appalachian west did the free lands and open environment of America begin to have decisive effects on the American character.²⁷

Since what was uniquely American did not develop during the seventeenth and first half of the eighteenth centuries, Turner and his followers never had much interest in the colonial period; it seemed irrelevant to the process of Americanization. In the colonial era the settlers remained essentially Europeans; only with the Revolution did the settlers become Americans and experience the effects

of the frontier. Consequently Turner's frontier history tended to concentrate in the nineteenth century, especially in the period from 1800 to 1850 when the Midwest was settled. The Turnerite school of history thus effectively intensified the remoteness and irrelevance of the colonial period to the history of America. The colonial period seemed to have little to say about the sources of what was peculiarly American.

The work of the third and most important school of professional historians writing during the first half of the twentieth century, the Progressive historians, had similar unintended effects. These historians, led by Carl Becker, Charles Beard, and Arthur Schlesinger, Sr., unlike the imperial historians, were interested in the origins and character of the United States. But they stressed the discontinuity of American history and were preoccupied with the ways in which the colonial era was different from subsequent American history. For them the Revolution marked a real turning point in American history, the uprising of popular forces against conservative aristocracies and the beginning of American democracy. In this perspective the colonial period that preceded the Revolution was not really American in character; it was instead English or European—a kind of ancien régime, undemocratic and quasifeudal and marked by elite rule, established churches, and a limited suffrage. In the eyes of the Progressive historians little that was truly American came out of the colonial era. They became interested in the period only so far as it helped to explain the Revolution, which they saw as a radical break from the past, and they consequently wrote very little about the seventeenth and early eighteenth centuries. Their writings suggested that anyone who wanted to understand the roots of American liberty and democracy really ought to begin with the Revolution. It was the Revolution that destroyed the older aristocratic order, expanded the suffrage and popular participation in government, and created the nation. When Charles McLean Andrews once reproached the Progressive historian Arthur Schlesinger, Sr., for ignoring the colonial era, Schlesinger in reply made just this point: the origins of the United States, he said, really lay with the Revolution, not with the colonial period.²⁸ With such a viewpoint the Progressive historians could not help but further separate the colonial period from what they took to be the real history of America.

All these developments during the first half of the twentieth century—the myth-making and sentimentalizing of the colonial era, the efforts by old-stock Anglo-Saxons to make the colonial period their special preserve, and the peculiar perspectives of the various schools of professional historians—all these led to a diminishing of professional interest in early American history. In their search for the roots of the nation, historians did not believe the colonial period had much to offer them.

This waning of interest did not show up at once. In the 1890s, at the beginning of professional history-writing in the United States, the colonial period still absorbed the attention of many scholars; but over the next half century it gradually gave way to other fields of interest, particularly to the more recent periods of United States history. More and more young talented researchers saw the colonial period as unimportant for what they wanted to say. In the first six volumes of the

American Historical Review between 1895–96 and 1900–1901 there were fifteen articles dealing with the colonial period of American history. The next six volumes between 1901–2 and 1906–7 saw this number drop to nine. Thereafter, despite all the substantial work of the imperial scholars like Osgood and Andrews, there began an irregular but unmistakable decline in the concern for early America as a subject of professional teaching and research. In the six volumes of the *American Historical Review* between 1907–8 and 1912–13 there were only five articles dealing with the colonial era. After a brief flurry of early American papers between 1914 and 1916, the number of articles on colonial history in the *American Historical Review* declined to one a year; in 1920–21 there were none. Over the next two decades the situation never really improved. By the time of World War II the colonial period was attracting very little of the historical profession's attention; in the six volumes of the *American Historical Review* between 1941–42 and 1946–47 there was only one article concerned with the first 150 years of American history.

This decreasing professional interest in early American history finally reached the point in 1948 where historian Carl Bridenbaugh felt compelled publicly to lament "the neglected first half of American history." Survey courses and textbooks in American history spent less and less time on the colonial period, sometimes omitting the first century and a half entirely. The numbers of new dissertations and publications in early American history had declined. Fewer young scholars were being trained in the subject. Most universities and colleges did not even offer courses in early American history. By the 1940s Johns Hopkins University, which had been the center of colonial studies in the late nineteenth century, no longer even had a historian working in the field. The same was true of other important graduate training centers such as Princeton, Pennsylvania, Chicago, and Berkeley.

Bridenbaugh blamed a number of developments for this neglect of early America, including an increased interest in presentism and relevance, lopsided textbooks, and the influence of Turner and the trans-Appalachian frontier at expense of the Atlantic seaboard. He hoped that the formation of the Institute of Early American History and Culture, of which he was then director, and the establishment of the third series of the *William and Mary Quarterly* in 1944 would help to turn things around. He and nine other distinguished scholars of early America met at Princeton in the spring of 1947 to devise ways and means for stimulating professional interest in colonial and Revolutionary history.²⁹ If what happened over the next several decades is any measure, they succeeded beyond their wildest dreams.

In the decades following 1947 everything changed. Colonial history became an important and flourishing field, not just of American history but of early modern Western history in general. Most universities and colleges now have courses in early American history, and some of the most distinguished historians in the country are specialists in the subject. In fact, three out of the past five presidents of the Organization of American Historians have been early Americanists, at least for a good part of their careers. Much of the most significant and pathbreaking

historical scholarship in America over the past generation has focused on the colonial period. In 1980 the principal journal in the field, *The William and Mary Quarterly*, was judged to be the most frequently cited historical journal in the world.³⁰

Following World War II many of the conditions that had existed at the beginning of the twentieth century were transformed. Most important in revitalizing colonial scholarship in the 1950s was the reemergence of a whiggish or Bancroftian view of American history. Amidst the Cold War atmosphere of the decades following World War II many Americans once again celebrated the long-existing role of the United States as the leader of the free world, destined by history to show people everywhere the way to liberty and democracy. What has been called an "exceptionalist" conception of America was never more widely proclaimed, as American historians, like Bancroft before them, began to see the whole of America's past pointing toward this providential role. All at once the first century and a half of American history became an integral part of the whole national story; the colonial past was relevant once again.

The historiography that had inhibited serious consideration of the colonial era was now evaded or overturned. One by one the works of historians written during the first half of the twentieth century were either bypassed or refuted. In the exceptionalist climate of the Cold War years Americans wanted to see their colonial past from their own side of the Atlantic; and the broad imperial perspective of Andrews and Osgood had to give way to newer, narrower, nationalist viewpoints. America was once again different from Europe, and it had been different from the beginning. The frontier theory of Turner that ignored the colonial era was attacked and modified out of importance; if it still had any meaning, that meaning was now made applicable to the colonial period as well as later periods of American history. And, most important, the works of the Progressive school of historians were undermined in dozens of different ways. Colonial society was not an *ancien régime* after all. There was no aristocracy in colonial America worthy of the name. The established churches were never as strong as the state churches in England or Europe. White male colonists could vote in larger proportions than any people in the world. Eighteenth-century America, historians of the 1950s like Robert E. Brown claimed, was already a middle-class democracy; the Revolution was merely a colonial rebellion, designed simply to preserve what had developed during the previous 150 years of colonial history. America, as Louis Hartz put it, echoing Tocqueville, had become free and equal in the migrations of the seventeenth century; it was liberal from the beginning of the colonial period. Suddenly the first half of American history was important once again in creating the sources of American democracy and nationhood.³¹

At the same time as the work of earlier schools of historians—work that had prevented serious consideration of the colonial period—was being dismantled, other circumstances that had cut the colonial era off from the rest of American history changed as well. The increasing ethnic diversity of the United States became such that efforts by a few Anglo-Saxons to claim some sort of special tie to the colonial era seemed more and more ludicrous. Organizations like the DAR

now had none of the importance in American life they had had during the early decades of the twentieth century. Antiquarian and colonial historical societies still existed, but many of them, such as the American Antiquarian Society, the Colonial Society of Massachusetts, and the Massachusetts Historical Society, were invaded by professional historians who had no genealogical roots in the colonial past or even in rural America. In the face of these changes the notion that the colonial period was simply a repository of old Yankee ancestors and customs could not be sustained.

Ironically, this emergence of the children and grandchildren of recent immigrants into the historical profession was lamented by Bridenbaugh in his presidential address to the American Historical Association in 1962. These urban-bred scholars, he said, were trying to write about a rural colonial past that they had no inherited connectedness with.³² It was a foolish lament. Not only was Bridenbaugh denying the very premise of all imaginative historical recovery, but he was ignoring the fact that it was precisely the post-World War II appearance in the profession of new historians without Anglo-Saxon backgrounds that was important to the revival of interest in early American history that Bridenbaugh himself had called for a decade and a half earlier. These new historians with varying ethnic backgrounds had no personal or emotional stake in the colonial period except as Americans, and therefore they tended to approach it disinterestedly and dispassionately.

As the character of the history profession changed and became more diverse in the decades following World War II, traditional political history gave way to social and cultural history, for which the colonial period had a natural receptivity. Indeed, much of the new American social and cultural history of the past thirty years was born in the colonial period. In the seventeenth and eighteenth centuries there are few headline political events to hang a conventional narrative line on: no presidents, no congresses, no supreme court decisions, no national elections to write about, which is one reason why the colonial period usually has seemed so mythical and free-floating, so easily telescoped and foreshortened. Yet during the past generation in which modern social and cultural history has flourished this lack of palpable political events and national institutions in the colonial period has become an advantage. The headline events and political institutions that preoccupied historians of the national period were not present to divert the attention of social and cultural historians. Colonial historians therefore were freer than national historians to concentrate on the *longue durée*, on social and cultural developments that go on longer than a few years or even a few decades. Only in the colonial period could historians of America have the long sweep of a century or more, uninterrupted by political events, in which to lay out long-term social and cultural developments. It is not coincidental that the modern study of American demographic and family history began first in the colonial period; or that one of the earliest studies of American attitudes toward death concentrated on the colonial period.³³ Historians could not trace such enduring social and cultural subjects over only a decade or two; they needed long stretches of time. Colonial historians, unlike national historians, had been used to dealing with

long periods of time and thus were more likely to be attracted to the new social and cultural history.

But the revival of interest in early American history was more than a matter of its being especially receptive to the new social and cultural history. Over the past thirty years the field has erupted and expanded in all directions. Early American history, according to Bernard Bailyn, the historian who over the past generation has dominated the period as much as any single scholar could, has experienced a "creative ferment of scholarship," resulting in "a wealth of research and writing concentrated on a relatively short period of time that is perhaps unique in western historiography."³⁴ The changes in the writing of colonial history have been phenomenal.

The impulses that initially lay behind the revival of early American history in the 1950s and early '60s have been transformed. Since the late 1960s American historians have become less and less interested in celebrating the uniqueness of the United States. The war in Vietnam if nothing else convinced many Americans that the moral character of the United States was not different from that of other nations and that the nation had no special transcendent role to bring liberty and democracy to the world. During the past several decades many American historians, if not the general public, have shed whatever faith they might once have had in the traditional idea of American exceptionalism.

These changes in outlook have been matched by equally important changes taking place in the society of the United States and in its historical profession. The new emphasis on diversity and the new racial, ethnic, and gender consciousness have tended to dilute a unified sense of American identity and have led to less and less emphasis on the nation as a whole in historical research and writing. All these changes have resulted in a shift in perspective on the colonial past; they have, in John Higham's words, "disconnected the colonial era from the narratives of American uniqueness or identity."³⁵

Once again the colonial period has lost much of its relevance for those historians looking for the origins or roots of the nation. But it has gained new relevance for those who have other questions or interests in mind. Recent historians, as Joyce Appleby says, have found it "easy to abandon the idea that what was truly important about the colonies was their contribution to American nationhood."³⁶ The consequence of this "gradual liberation from the nationalist paradigm" has been a fundamental shift in the nature and purposes of colonial historiography.³⁷

Most obvious has been a reappraisal of the discontinuity between the colonial and national periods of American history. The roots of modern democratic America cannot be found in the colonial era after all. America, it seems, was not born free, equal, and liberal in the seventeenth century; like the nations of Europe, it had to become so, and this apparently did not happen until the nineteenth century. Some colonial historians like James Horn and David Hackett Fischer have explicitly challenged the "paradigm of exceptionalism," which holds that "colonial society diverged significantly from its parent culture." Instead, they have stressed the continuities between Europe and America and the

“contribution of Old World cultures to New World society”; in fact, Fischer recalls the scholarship of Herbert Baxter Adams in arguing for a “modified ‘germ thesis’” in his account of the perpetuation of British folkways in colonial America.³⁸ Britons in colonial America were still Britons. The traditional patriarchal forms and values of the Old World persisted in the New—in family life, society, and politics. Seventeenth- and eighteenth-century colonial America, it seems, was a kind of *ancien régime* after all. The established churches, weak as they may have been, still gave a statist tone to the culture. Social hierarchies and familial politics aped those of the mother country. The few dominated the many, and, as in Europe, ordinary people had little influence over the course of public life. Most common farmers in colonial America were content to live out their lives as their parents had; they seemed no more individualist or capitalist than their European counterparts.³⁹

But more important in changing the historiography of colonial history than the reappraisal of discontinuity between the colonial and national eras has been the remarkable ways some historians of early America have enlarged their perspective—no longer focusing exclusively on the territory that became the United States but, as Bernard Bailyn has put it, seeing early modern worlds in motion from a satellite hovering somewhere over the Atlantic.⁴⁰ A century ago Herbert L. Osgood hoped eventually to see an American colonial history that “will be taken out of its isolation and will appear as a natural outgrowth of the history of Europe.”⁴¹ Historians of the past several decades have been well on the way not merely to fulfilling Osgood’s hope but to surpassing it. The colonies are now seen as an outgrowth not just of Europe but of Africa as well. Historians have greatly broadened the boundaries of what constitutes early American history, almost to the point where they now seem limitless. For many historians early American history is no longer what it was for Bancroft and most historians of the 1890s, a means for understanding the origins of the United States; it has become an important and vital part of the pan-Atlantic world in the early modern era.⁴² As nationhood has receded in importance, historians have become less interested in early America for its own sake and more for what it reveals about the great transformation from premodern to modern society.

American colonial history has not only become part of western European and African history, but it now seems to make sense only as it embraces the peoples of Hispanic America as well.⁴³ Some early American historians have called for entirely new conceptions of the colonial past, new conceptions that would “think of colonial history as a history of all of the Americas,” and that would reintroduce the hemispheric perspective that Herbert E. Bolton tried and failed to make stick in the 1930s—a perspective that placed United States history in a comparative framework with Canada, Mexico, the Caribbean, and the countries of South America. These efforts to make the history of Santa Fe in 1776 just as important as the history of Boston in 1776 are not the idle chatter of a few multicultural-minded historians.⁴⁴ In fact, the Omohundro Institute of Early American History and Culture in Williamsburg, the center for early American studies in America and the publisher of the *William and Mary Quarterly*, has recently announced that

it plans "to diversify its agenda." Without abandoning its traditional commitment to studying the British North American colonies, "Institute publications and programs are currently attempting to embrace a larger range of subjects, including especially the peoples of Hispanic America and West Africa."⁴⁵ Early American historians now have concerns other than the origins of the United States.

As historians have lost confidence that the United States has a collective identity with common origins, they have shifted their focus away from the nation as a whole and have zoomed in to the histories of groups or individuals within the larger community. Instead of writing about the nation or the British empire or even single colonies, many have concentrated on towns or counties or even obscure single families. Indeed, the most interior and private aspects of daily life have now become open to serious historical study—what the colonists ate, how they treated their children, how husbands and wives related to each other, how they managed their emotions. By trying ever harder to recapture the private spaces and personal lives of ordinary people, historians have written ever smaller and more intimate snatches of history, some of which, like Laurel Thatcher Ulrich's *A Midwife's Tale* and John Demos's *The Unredeemed Captive*, are truly marvelous to behold.

All these private and personal histories, however, were just aspects of a larger attempt at historical retrieval. In their earlier search for the origins of American identity, most historians of colonial America had tended to ignore the voices of a variety of peoples—slaves, artisans, women, and Indians—who did not appear to contribute to the traditional history of the exceptionalist nation. Since the 1960s all this has changed. By playing down America's collective identity and emphasizing its pluralism, the new social and cultural history of the past several decades has recovered many of these lost voices.

The shift of perspective from the sources of American nationhood to the larger early modern world has made the Indians especially visible.⁴⁶ Because of their preoccupation with the origins of America's peculiar national character, earlier historians had not been able to see the Native Americans with any clarity. Indeed, historians like Turner had scarcely acknowledged the existence of the Indian. For Turner the New World the Europeans came to in the seventeenth century was "virgin soil," an "unexploited wilderness" out of which American distinctiveness was born; it was "the fact of unoccupied territory in America that sets the evolution of American and European institutions in contrast."⁴⁷ As Louis Hartz pointed out long ago, this neglect of the Indian in early American historiography stemmed solely from the "interior perspectives" of historians like Turner. Since it was the fate of America "to destroy and exclude the Indian, life inside it has had a dwindling contact with him. How could he then be perceived? How could he be appreciated as a problem comparable to the rise of the 'common man' or the emergence of the trusts?" But, of course, Hartz said, once American historians get outside the narrow confines of the nation, "the very fact that the Indian was thus eliminated . . . becomes a matter of very great importance."⁴⁸

Today historians who have sought to get outside the national history of the United States have a very different appreciation of the presence of the Indians.

During the past decade or so the numbers of books and articles on the native peoples of North America in the colonial period have multiplied dramatically. In the fifteen years of the *William and Mary Quarterly* between 1959 and 1973 only four articles on Indians appeared. But in the fourteen years between 1974 and 1988 there were twenty articles dealing with Native Americans. And since 1988 the number of contributions to what has come to be called "ethnohistory" has increased even faster. Indeed, as Ian Steele has recently said, the "field of ethnohistory . . . is developing so quickly that any attempt at accessible synthesis is bound to be premature and incomplete."⁴⁹ Some of the best and brightest historians in the United States have been turning to the Indians as a subject of research, and books on Indians in early America have begun winning prestigious prizes.⁵⁰

Presumably these recent sensitive studies of the Indians in the colonial period have been a consequence, as Appleby has put it, of "the quickening of interest in non-Western cultures that came with the diminished credibility of the West's claims to be directing the path of human destiny."⁵¹ The question now to be faced is whether such a "quickenning of interest in non-Western cultures" by professional historians will eventually weaken popular interest in early American history. With colonial historians presently being urged to "avoid letting their field again become the prehistory of the United States," will citizens of that United States continue to be much interested in what these new colonial historians write?⁵² There may be a limit to the degree to which the American people will put up with having a colonial history that pays no attention to the nation, "except," in John Higham's words, "as a villain in other people's stories."⁵³ Although few historians these days would write a history the way George Bancroft did, with, in Frank Craven's words, all "his willingness to let love of country illuminate the text," outright hostility to the country may not sit well with the American public.⁵⁴ Historians who see their role as simply being "a critic of the culture" whose principal task is "to illuminate conditions of the present by casting a harsh light on previous experience" may not be able to develop much of a popular following; most people do not seem very eager "to learn unpleasant lessons from their study of the past."⁵⁵ If the colonial era is to be simply an arena for criticizing American culture and is to be cut loose from the story of American nationhood or identity, will it continue to be meaningful to most Americans?⁵⁶ The old question of the relevance and irrelevance of the colonial past to American history seems once again on the table.

NOTES

1. Wesley Frank Craven, *The Legend of the Founding Fathers* (Ithaca, N.Y., 1956), 63: "Of these colonies," wrote George Chalmers in 1780, "it cannot be asserted, as it is of European nations, that their origin is uncertain or unknown: that their ancient history is fabulous and dark; or that their original institutions have come down the current of time, loaded with the disputations of the antiquary."

2. One textbook that collapsed time in this way was T. Harry Williams, Richard N. Current, and Frank Friedel, *A History of the United States* (New York, 1959), I, 95–96.

3. Adams, "Dissertation on the Feudal and Canon Law" (1765), in Gordon S. Wood, ed., *The Rising Glory of America, 1760–1820*, rev. ed. (Boston, 1990), 31.

4. Jack P. Greene, *The Intellectual Construction of America: Exceptionalism and Identity from 1492 to 1800* (Chapel Hill, 1993), 199.

5. For an illuminating account of the cultural politics of historical memory in the early republic, focusing on literary and historical accounts of Puritanism in colonial New England, see Philip Gould, *Covenant and Republic: Historical Romance and the Politics of Puritanism* (Cambridge, Eng., 1996).

6. Gould, *Covenant and Republic*, 17.

7. Gould, *Covenant and Republic*, 21.

8. Gould, *Covenant and Republic*, 29.

9. Craven, *Legend of the Founding Fathers*, 98.

10. Greene, *The Intellectual Construction of America*, 175; David D. Van Tassel, *Recording America's Past: An Interpretation of the Development of Historical Studies in America, 1607–1884* (Chicago, 1960), 69, 81.

11. George Bancroft, *History of the United States, from the Discovery of the American Continent* (Boston, 1853), I, vii.

12. Dorothy Ross, "Historical Consciousness in Nineteenth-Century America," *American Historical Review* 89 (1984): 915–19.

13. Bancroft, *History of the United States*, I, vii, 74.

14. Van Tassel, *Recording America's Past*, 117; Craven, *Legend of the Founding Fathers*, 133.

15. William Taylor, *Cavalier and Yankee: The Old South and American National Character* (New York, 1961, 1969).

16. Craven, *Legend of the Founding Fathers*, 158.

17. Craven, *Legend of the Founding Fathers*, 114–18.

18. Craven, *Legend of the Founding Fathers*, 158.

19. Craven, *Legend of the Founding Fathers*, 149–50, 157.

20. Celia Betsky, "Inside the Past: The Interior and the Colonial Revival in American Art and Literature, 1860–1914," and William B. Rhoads, "The Colonial Revival and the Americanization of Immigrants," in Alan Axelrod, ed., *The Colonial Revival in America* (New York, 1985), 242, 349.

21. John Higham, *History: Professional Scholarship in America* (Baltimore, 1983), 160–61; John Higham, "Herbert Baxter Adams and the Study of Local History," *American Historical Review* 89 (1984): 1225–39.

22. Jameson, quoted in Higham, "Herbert Baxter Adams," 1235.

23. Jameson, "The American Historical Review, 1895–1920," *American Historical Review* 26 (1920–21): 1.

24. Woodrow Wilson to J. H. Kennard, Jr., Nov. 18, 1884, quoted in Bernard Bailyn, *The Origins of American Politics* (New York, 1968), vii–viii.

25. Herbert L. Osgood, "The Study of American Colonial History," *Annual Report of the American Historical Association for the Year 1898* (Washington, D.C., 1899), 63–73; Charles M. Andrews, *The Colonial Background of the American Revolution: Four Essays in American Colonial History* (New Haven, 1924), 175, 178.

26. Andrews, quoted in Michael Kraus, *The Writing of American History* (Norman, Okla., 1953), 242.

27. Frederick Jackson Turner, "Western State-Making in the Revolutionary Era," *American Historical Review* 1 (1895-96): 70

28. Higham, *History*, 187.

29. Bridenbaugh, "The Neglected First Half of American History," *American Historical Review* 53 (1947-48): 506-17.

30. According to an index of over a thousand scholarly journals for the year 1980 compiled by the Institute for Scientific Information, the *William and Mary Quarterly* was judged to be the most frequently cited journal of historical scholarship in the world, during that one year at least. See *A News Letter from the Institute of Early American History & Culture* 73 (Mar. 1, 1983), 73.

31. For a summary of this 1950s scholarship see Bernard Bailyn, "Political Experience and Enlightenment Ideas in Eighteenth-Century America," *American Historical Review* 67 (1961-62), 339-51.

32. Carl Bridenbaugh, "The Great Mutation," *American Historical Review* 68 (1962-63): 315-31.

33. Philip Greven, "Historical Demography and Colonial America," *William and Mary Quarterly*, 3rd ser., 24 (1967): 438-54; David E. Stannard, *The Puritan Way of Death: A Study in Religion, Culture, and Social Change* (New York, 1977). Much of what follows draws on my article "A Century of Writing Early American History: Then and Now Compared; or How Henry Adams Got It Wrong," *American Historical Review* 100 (1995): 678-96.

34. Bernard Bailyn, *The Peopling of British North America: An Introduction* (New York, 1986), 6.

35. John Higham, "The Future of American History" *Journal of American History* 80 (1994): 1298.

36. Joyce Appleby, "A Different Kind of Independence: The Postwar Restructuring of the Historical Study of Early America," *William and Mary Quarterly*, 3rd ser., 50 (1993): 249; Higham, "Future of American History," 1298; Michael McGerr, "The Price of the 'New Transnational History,'" *American Historical Review* 96 (1991): 1066.

37. Mathew Mulcahy and Russell R. Menard, "Comment on 'Why the West Is Lost,'" *William and Mary Quarterly*, 3rd ser., 51 (1994): 741.

38. James Horn, *Adapting to a New World: English Society in the Seventeenth-Century Chesapeake* (Chapel Hill, 1994), 9, 10; David Hackett Fischer, *Albion's Seed: Four British Folkways in America* (New York, 1989), 4-5.

39. For a review of a number of these works see Gordon S. Wood, "Inventing American Capitalism," *The New York Review of Books*, vol. XLI, no. 11 (June 9, 1994), 44-49.

40. Bailyn, *Peopling of America*, 3.

41. Dixon Ryan Fox, *Herbert Levi Osgood: An American Scholar* (New York, 1924), 72.

42. Bernard Bailyn, "The Idea of Atlantic History," *Itinerario* 20 (1996): 1-27.

43. Bernard Bailyn, "The Challenge of Modern Historiography," *American Historical Review* 87 (1982): 2; A. Roger Ekirch, "Sometimes an Art, Never a Science, Always a Craft: A Conversation with Bernard Bailyn," *William and Mary Quarterly*, 3rd ser., 51 (1994): 657.

44. Mulcahy and Menard, "Comment on 'Why the West Is Lost,'" 743; Herbert E. Bolton, "The Epic of Greater America," *American Historical Review* 38 (1933): 448-74.

45. James A. Hijiya, "Why the West is Lost," *William and Mary Quarterly*, 3d ser., 51 (1994): 276-92; "Why the West Is Lost": Comments and Response," forum in *ibid.*, 717-54.

46. Appleby, "A Different Kind of Independence," 260.

47. Turner, "Western State-Making," 70-72.

48. Louis Hartz, *The Founding of New Societies: Studies in the History of the United States, Latin America, South Africa, Canada, and Australia* (New York, 1964), 94.

49. Ian K. Steele, *Warpaths: Invasions of North America* (New York, 1994), preface.

50. For an able summary of historiography about the Indians over the past hundred years see R. David Edmunds, "Native Americans, New Voices: American Indian History, 1895–1995," *American Historical Review* 100 (1995): 717–40.

51. Appleby, "A Different Kind of Independence," 259.

52. Mulcahy and Menard, "Comment on 'Why the West Is Lost,'" 743.

53. Higham, "The Future of American History," 1298.

54. Craven, *Legend of the Founding Fathers*, 100. In a 1994 op-ed piece Richard Rorty warned of the dangers for an academic left that, "in the name of 'the politics of difference,' . . . refuses to rejoice in the country it inhabits [and] repudiates the idea of a national identity, and the emotion of national pride." Such an academic left, said Rorty, "will become increasingly isolated and ineffective. An unpatriotic left has never achieved anything. A left that refuses to take pride in its country will have no impact on that country's politics, and will eventually become an object of contempt." Rorty, "The Unpatriotic Academy," *The New York Times*, Feb. 13, 1994. On this point see also Todd Gitlin, *The Twilight of Common Dreams: Why America Is Wracked by Culture Wars* (New York, 1995).

55. Daniel K. Richter, "Whose Indian History?" *William and Mary Quarterly*, 3rd ser., 50 (1993): 388.

56. Already, for example, historians of the Indians have begun lamenting that their professional new Indian scholarship, with its "oppositional perspective on the dominant culture," has not had much impact on the popular mind. Consequently, some of them have begun trying to stress the contributions of the Indians not only to the origins of American society and culture but also to the beginnings of American democracy and the formation of the Constitution. Richter, "Whose Indian History?" 380, 386, 388, 389. For the Indians' contributions to American democracy and the Constitution see Oren Lyons et al., eds., *Exiled in the Land of the Free: Democracy, Indian Nations, and the U.S. Constitution* (Santa Fe, 1992). For a debate on the issue see "Forum: The 'Iroquois Influence' Thesis—Pro and Con," *William and Mary Quarterly*, 3rd ser., 53 (1996): 587–636.

Nineteenth-Century American History

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IN THEIR SEARCH for the central theme of nineteenth-century American history, historians have nominated several candidates, some of which seem better qualified for the office than others. The traditional, self-congratulatory emphasis on “the rise of democracy” might serve for the first half of the century, but only if one forgets the fact that equal citizenship and political participation were strictly limited to white males. Since recent historians have tended to focus on the experiences of groups excluded from “the people” as defined by Jacksonian democrats—especially blacks, Indians, and women—the once-popular view of pre-Civil War American progress toward democratic perfection has few adherents among recent historians. For the post-Civil War era the democratization theme becomes even more problematic, despite the emancipation and extension of putative citizenship to African-Americans. By the end of the century, most historians would agree, new hierarchies of power and authority associated with corporate capitalism were threatening the democratic and egalitarian ethos that had developed earlier among the white male members of a society of yeomen farmers and small producers. Under such circumstances, the popular politics of mass meetings and high rates of voter participation was being replaced by a political system featuring appointed commissions, organized pressure groups, and lower rates of voter turnout.¹

Implicit in much recent scholarship in social and labor history is a view of nineteenth-century development that comes close to turning the theme of democratic progress on its head. Its point of departure is a radical republican tradition originating with the left wing of the American Revolution and articulated in differing ways by thinkers like Thomas Paine and Thomas Jefferson. Its conception of equal rights and personal independence as essential to liberty inspired resistance on the part of artisans and yeomen farmers to the growth of commercialism, industrial wage labor, and the concentration of capital, a struggle that began with the workingmen’s parties and agrarian radicals of the Jacksonian era and ended with the demise of the Knights of Labor and the Populist party in the 1890s. A grand synthesis of nineteenth-century American history along these lines would be challenging and provocative but might be open to the criticism that it idealizes a white male tradition that, for the most part, condoned the subordination of women, blacks, and Native Americans. Furthermore, its insistent and pessimistic antimodernism is likely to prove unacceptable to the many

historians who continue to believe in the possibility of progressive reform within the framework of democratic capitalism.²

A candidate for central theme that assumes a similar pattern of change but is less ideologically charged (and pays close attention to winners as well as losers) comes from the historians of American society who emphasize broad patterns of socioeconomic change. For them the nineteenth century was preeminently the time of a great transformation from the distended society of families and communities dependent on agriculture, localized commerce, and artisan labor that existed in 1800 to the centralized, organized, and incorporated urban-industrial nation of the early twentieth century.³

Such changes can scarcely be denied, but to make virtually everything that happened a manifestation or reflection of deep societal transformation tends to foster a linear determinism that obscures the human agency, shifts in direction, and historical contingencies to which historians have become increasingly sensitive. It also deprives politics and government of the degree of autonomy that many historians, following recent trends in political science and political sociology, would like to assign to them. An exclusive preoccupation with this great transformation is likely to end historiographic discussion and debate rather than open it up, unless one asks how the juggernaut of socioeconomic development affected, or was effected by, specific groups, ideas, and events. Although it is obviously true in some sense, the "modernization" theme is too abstract and teleological to satisfy the majority of contemporary historians. It homogenizes too much experience that the United States shares with many other nations and does not encourage attention to what may be special or unique about the American past. One does not have to be an "American exceptionalist" in the full sense of considering the United States radically different from all other countries to relish America's peculiarities or at least its variations on the general themes of international history.

One way of differentiating American development from that of most other nations that industrialized and urbanized during the nineteenth century would be to follow in the footsteps of Frederick Jackson Turner by stressing the significance of the moving frontier. A westward movement that transformed the United States from an Atlantic seaboard republic into a continental one between 1815 and 1890 was clearly a major aspect of the nineteenth-century story. But recent historians have not assigned to the frontier the kind of fundamental importance given to it by Turnerians. Rather than spawning a new and unique American culture of democratic individualism, as Turner claimed, the frontier is now more commonly viewed as an arena in which forces and tendencies emanating from the settled East came into collision—for example sectional differences over the extension of slavery and conflicts between agrarians and commercial or industrial capitalists over the terms of trade in a market economy. The "new western history," emphasizing the Native American and Hispanic sides of the conflict over western land, makes the frontier the scene of unresolved racial and ethnic conflicts rather than the main source of consensual American values.⁴

The theme of nineteenth-century history that preoccupies the largest group of historians is the social, political, and ideological conflict that divided the nation and was resolved through sectional war and the subsequent reconstruction of the Union. The Civil War theme—broadly defined to include not only the war itself but also the story of slavery and the Old South, the sectional controversy over the expansion of slavery that led to secession from the Union, and the turbulent process of national reunification that took place after the war—has inspired more scholarship than any other nineteenth-century subject. One of its attractions is that it provides a persuasive argument for the uniqueness of American history that is not based on some claim to special virtue. Other nations experienced the trauma of economic and social modernization and saw the ebb and flow of democratic reform (some like Russia and Brazil even had moving frontiers), but none fought a devastating internal war over slavery. The apparent limitation of this emphasis is that it does not, at first glance, seem quite valid for the whole century but appears to lose its centrality in the 1870s.

This essay will focus mainly on how recent historians have dealt with the origins and impact of the Civil War, and it will argue that new work permits us to view the legacies and aftershocks of the war as lying at the root of many of the problems of the 1880s and '90s. It will also attempt to show the relevance of this historiography to much of the scholarship concerned more directly with the great transformation to modernity and advanced capitalism—even when that scholarship does not explicitly deal with the causes and consequences of the war.

Most historians of the United States would agree that the Civil War was the central event of the nineteenth century, if not of the nation's entire history. The period between 1789 or 1815 and the firing on Fort Sumter in 1861 is commonly referred to as the antebellum era, and the years between 1865 and the turn of the century are perhaps best designated as the post-Civil War period. The more common label for the century's last three decades—"the Gilded Age"—evokes certain tendencies of that era, but is perhaps less useful than one that calls attention to the shadow of the war. It was not, for example, until the 1890s that the race question that had to be faced when millions of African-Americans were emancipated from slavery was settled by the South's legalization of the new system of mandatory segregation, making a full reconciliation of the North and South possible. It was also in that decade that the bitter political struggle over the inflation or contraction of the currency, set in motion by the issuing of unredeemable "greenbacks" during the war, was resolved by the defeat of "free silver" in the election of 1896.

American historians have devoted massive and sustained attention to the Civil War. A debate on the causes of the war has been going on since Appomattox, and the issue plays a role in American historiography equivalent to that of the origins of the French Revolution in European historical studies. The war years themselves have received even greater attention if one can judge from the sheer number of books published, but much of that work has been narrowly conceived military history that bears little relationship to the central issues of nineteenth-century American historiography. The fixation on narratives of battles and cam-

paings has had the paradoxical effect of detaching the war from the mainstream of American history by plunging readers into a special world of intense experience and heroic action that has little apparent connection with the processes that affected people's lives and determined public policies over an extended period.⁵

Despite the tendency to an antiquarian form of narrative history in much of the writing about the war itself, a number of scholars have addressed the question of the war's impact on postwar America—most obviously in the burgeoning literature on Reconstruction. General works on the immediate postwar efforts to reunite the nation and determine the status of the ex-slaves often begin with what happened during the war itself and quite properly portray peacetime Reconstruction efforts as a continuation of wartime initiatives, especially the Emancipation Proclamation and the enlistment of black soldiers in the Union army. But the Reconstruction era is thought to have ended in 1877, and the question of the more enduring legacies of the war has been relatively neglected.

In fact there is a tendency to periodize American history in such a way as to create the impression that the war ceased to matter very much after the 1870s. When a publisher brings out a series of books covering all of American history, it is common practice to have a volume on the Civil War and Reconstruction, covering a period between the 1840s and the 1870s, followed by an offering on the late nineteenth and early twentieth centuries. The mid-nineteenth-century volume commonly reaches back into the early national and Jacksonian periods in search of the roots of the sectional conflict, but its successor is likely to ignore the consequences of the war entirely, implying that the last two or three decades of the nineteenth century were a prelude to the "Progressive period" of the early twentieth century rather than an extension of the postwar era. There is of course nothing sacred about the popular notion that history follows the calendar and that decades, centuries, or millennia have more than a conventional or heuristic significance. But it seems arbitrary and inconsistent to trace the war's origins back a half century or more and then ignore any legacies that may have lasted more than a decade.

The historiography of the war's causation has been the subject of innumerable essays and even one major book.⁶ The way historians have conceived the war's impact has received less attention from historiographers, except to the extent that the results can be inferred from a conception of the causes. The possibility that the conflict had significant accidental or unintended consequences has not received the attention it deserves.

Recent treatments of the background, course, and aftermath of the Civil War have naturally been influenced by general trends in American historiography—new conceptions of what is important and what methods are needed to study it. Perhaps the most important of these is the vogue of social history or "history from the bottom up." A deemphasis on the ideas and actions of elites and a preoccupation with the "agency" of ordinary people and "subaltern" groups is reflected in studies of the struggles of African-American slaves for cultural and psychic survival under a harsh form of servitude and of the role they played in bringing about their own emancipation. Also receiving increased attention are the parts

played by women and gender issues in the sectional crisis and its resolution, the experiences of common soldiers and home-front workers during the war, and the role of the freedpeople in the politics and socioeconomic adjustments of the Reconstruction era.⁷

These studies have enlarged our sense of who made history, but they have not obviated the need to study those who possessed power and were in a position, most of the time, to command the obedience of others. Although somewhat out of fashion among academic historians, many excellent studies of the ideas and actions of the men in command have appeared recently, and three of them have won Pulitzer prizes.⁸ History from below has been a useful corrective to the traditional focus on elites, but it risks becoming a romantic evasion of historical reality if it is not accompanied by an acknowledgment of the power—exercised within limits that have to be determined by empirical investigation—that influential groups and prominent individuals have been able to exert over the lives and attitudes of those who have lacked the same access to physical resources, political leverage, and social prestige. It remains true that slaves by themselves did not overthrow slavery, although they did play a role in its demise. Common soldiers could not choose when and where to fight, although how they fought could determine the outcome of a battle. Women could not vote, run for office, or bear arms, but they could influence the behavior of the men who did. Freed slaves had some influence over politics and government in the South during Reconstruction, but in the end they lacked the power to block a white-supremacist counter-revolution.

A second trend that has affected the historiography of the sectional crisis and the Civil War has been the more subtle shift from a behaviorist approach to history to one that emphasizes the interpretation of culture. Two decades ago the cutting edge of historical scholarship on the nineteenth century seemed to come from quantitative work on social, economic, and political behavior. Some historians are still encoding data on voting patterns, legislative roll calls, social mobility, household composition, and the prices of land, labor, and commodities. But, as anyone who has directed doctoral dissertations in history departments in recent years can probably testify, this is not what interests most younger scholars. Quantitative history or “cliometrics” has become more than ever the preserve of economists, political scientists, and behavioral sociologists. Historians are now more likely to look to cultural anthropology or to the cultural studies movement in literary scholarship for interdisciplinary inspiration.⁹

The general trend toward looking at the past through the lenses of culture and language is evident in studies of slave folklore and religion, of political rhetoric (especially the legacy of Revolutionary-era “republicanism”), and in the discourse on the meaning of the war to be found in soldiers’ letters home or in the journals and other private writings of civilians.¹⁰ Objective conditions that can, as it were, be weighed and measured have become less important to many historians than the subjective states to be found in the “discourse” of historical actors. This trend has led to a deeper understanding of what people thought was at stake in the sectional quarrel and in the reunion process that followed, but it has also tended

to obscure some of the structural factors that may have predisposed people to express themselves in the way that they did.

It is far from the case, however, that the new cultural history associated with the “linguistic turn” has eclipsed other paradigms in the historiography of the pre- and post-Civil War eras. The most influential scholars in the field continue to be those whose inspiration derives more from the Marxian or Gramscian tradition of class analysis and political economy than from nonmaterialist cultural theory (see especially the work of Eugene Genovese, Eric Foner, and Barbara Jeanne Fields). The most important work of these scholars has affirmed the reality of social class as an underlying determinant of cultural and ideological formations, including those associated with slavery and racial domination.¹¹

Others have drawn on the Weberian tradition in sociological thought to make the argument that “race” played an autonomous role in the making of social dominance and in the construction of ideologies to defend or oppose it. Unlike idealist historians who make racism simply an enduring cultural trait of white Americans, the neo-Weberians view the essence of race as simultaneously structural and cultural; it is a form of what Weber called “ethnic status,” and is fundamentally a hierarchical social relationship based on a differential assignment of honor and prestige to groups that differ in ancestry; only secondarily is it the specific set of racial stereotypes and images that is used to rationalize the subjugation of a particular subaltern group. The relationship between “race” and “class” is obviously a central and enduring problem for historians of the nineteenth-century South and its relationship to the rest of the nation.¹²

Strange as it may seem, the latest varieties of social and cultural history have not as yet had a decisive effect on how historians deal with the big question of what caused the Civil War. The reason for this might be that large questions of causation are alien to these specializations. Social history is at its best in dealing with small communities rather than whole nations, and those forms of cultural history that stress the interpretation of “texts” may dispense entirely with the study of causation, which normally requires attention to conditions or factors that are thought to have an existence independent of, and prior to, the recoverable discourses of historical actors.

An analysis of the preconditions and triggering events of an enormous convulsion like the American Civil War requires the talents of that increasingly rare breed—the historical generalist. It is not surprising therefore that the most valuable and authoritative discussions of the background of the war are still those that were written by distinguished members of the last great generation of eclectic general historians, men who came of age in the 1930s and '40s like David M. Potter and Kenneth M. Stampp.¹³ Younger scholars who have rivaled them in the breadth and boldness of their discussions of the origins of the conflict are likely to be those, such as Eugene Genovese and Eric Foner, whose work is influenced by the Marxian tradition of class analysis, which of course generates its own special conception of general history.¹⁴

If there is one thing that the eclectic and neo-Marxian generalists have agreed upon, it is that slavery was at the root of the sectional conflict. In the words of

David Potter, "slavery really had a polarizing effect, for the North had no slaveholders—at least not of resident slaves—and the South had virtually no abolitionists. . . . slavery had an effect which no other sectional factor exercised in isolating North and South from each other."¹⁵ This is not a discovery of modern historians and in fact merely repeats what has always been the most common—and commonsensical—explanation of the crisis leading to the Civil War. James Ford Rhodes, the premier late-nineteenth-century historian of the war period, put it succinctly: "The question [of causation] may be isolated by the incontrovertible statement that if the negro had never been brought to America, our Civil War could not have occurred."¹⁶

Potter's statement was historiographically significant because it signified his departure from the school of "revisionist" historians to which he himself had originally belonged. The revisionists, who were particularly influential in the 1930s and '40s, had viewed the war as an unnecessary conflict brought about by a combination of demagoguery, fanaticism, and political blundering. Slavery could not have been the real issue, they had maintained, because it was on its way to peaceful demise in 1860, and its sudden abolition was not worth the price in white lives that wartime emancipation had entailed.¹⁷

In the 1950s and '60s, historians and economists were finding the slave economy robust and unlikely to have collapsed under its own weight in the late nineteenth century. But a more significant reason for the decline of revisionism during the civil rights era was the racial insensitivity that it reflected. Historians of slavery as an institution were in the process of repudiating the previously ascendant view that the slave regime was a benign one that benefited blacks by "civilizing" them. If the regime was as brutal as Kenneth Stampp and Stanley Elkins claimed, and if blacks were not "natural children," inherently inferior to whites, the willingness of historians to countenance a few more decades of servitude as the price for avoiding civil war seemed downright immoral.¹⁸

But a modified form of revisionism survived in the work of those quantitative historians who analyzed political behavior and alleged that the northern voters and politicians who made the decisions that led to war did not do so because they shared the antislavery and antiracist convictions of modern liberal historians. Making slavery a "necessary cause" of the crisis did not of course explain why sectional division became unmanageable in the 1850s rather than sooner or later. Historians have generally agreed that the direct antecedent of secession and Civil War was the breakdown in the 1850s of the system of bisectonal political parties that had developed in the 1830s. The new political historians claimed that the breakdown occurred, not so much because of ideological differences on the slavery issue, as because of the ethnoreligious tensions resulting from mass immigration. Whether or not the war was necessary to rid the nation of slavery, they argued, it could not be proved that differing attitudes toward slavery provoked the crisis that led to its abolition. In support of this hypothesis, they pointed to the widespread racism and indifference to black freedom that prevailed in the North and to the undeniable fact that the abolitionists were an

unpopular minority until the exigencies of war provided a pragmatic justification for emancipation.¹⁹

A standard criticism of the neorevisionist "ethnocultural" explanation of political breakdown was that it had difficulty explaining why the collapse of the Whig party in the early-to-mid 1850s led to the rise of the Republicans and the sectionalization of politics over the issue of whether slavery should be allowed to spread to the federal territories. A more plausible outcome from this perspective would have been a realignment in which the antiimmigrant Know Nothing party would have survived as the alternative to the Democrats (who stood for a tolerant attitude toward non-English newcomers and the growing Catholic minority) instead of enjoying only an ephemeral success. Eric Foner's seminal study of Republican ideology, which appeared in 1970, described and analyzed the party's "free labor" ideology and its representation of the threat to the northern way of life represented by an expansionist "slave power," thus providing a plausible basis for making antislavery conviction, of a kind that was rooted less in pure idealism than in the class consciousness and perceived interests of a middle stratum of northern society, the key to northern sectionalism.²⁰

More recently, William Gienapp has attempted with some success to synthesize elements of the ethnocultural explanation of party breakdown with Foner's ideological explanation of northern sectionalism by arguing that some of the same cultural biases that inspired nativism also helped to fuel the "free soil" movement and inspire vigorous resistance to "the slave power conspiracy."²¹ Gienapp's work suggests that a weakness in Foner's interpretation of the rise of the Republicans is that it pays too little attention to the role of religion and culture in the worldview of the party's adherents.

Explaining why a sectionalist party emerged in the North in the late 1850s does not fully account for why the southern states seceded in response to the election of a Republican president in 1860, thereby dividing the Union and making Civil War likely if not inevitable. No recent historians of any stature have doubted that fears for the future of slavery were at the heart of southern concerns, but they have differed markedly on the question of precisely why the overwhelming majority of white southerners were so desperately committed to the defense of their "peculiar institution" that they were willing to go to war to defend it from any conceivable threat. In its essentials, the ongoing debate is between a "class" and a "race" interpretation of southern separatism. According to the neo-Marxian class interpretation put forth by Eugene Genovese and his followers, a premodern, precapitalist ruling class led the South into a war for its independence because it feared being dominated by the "bourgeois" elements that were coming to power in the North. This viewpoint makes the Civil War a bourgeois revolution in the European sense because it established the dominance of an emerging industrial capitalist class over an American equivalent of the landed aristocracies who resisted the rise of the bourgeoisie on the other side of the Atlantic.²²

The critics of this interpretation—a prominent recent example is James Oakes—first of all deny that the South was genuinely precapitalist and thus deny

that it departed categorically from the liberal and republican values that had inspired the American nation since its founding. The South's dominant ideology, according to Oakes, was less an attempt to repudiate everything that the North stood for than a desperate effort to reconcile slavery with its own persistent commitment to personal rights, economic individualism, and representative government.²³ The main device that permitted the coexistence of slavery with liberal and republican values was racism. If blacks were indeed subhuman there was no need to include them in the social contract and the rights-based polity that it sanctioned. Without denying that economic self-interest was a major source of proslavery commitment, this interpretation makes the fear of a loss of racial control and the status advantages that all white southerners—not just the minority that owned slaves—derived from black subordination the main element in the “crisis of fear” that simmered in the late 1850s and was brought to a boil by Lincoln's election.²⁴

If Genovese's view of southern ideology complements Foner's class-based conception of the ideological sources of northern sectionalism, the alternative emphasis on race status and control has some affinity with the modified ethnocultural view of northern sectional politics put forth by Gienapp. A claim for the decisive influence of culture and ethnic or racial identities on many of the historical actors of this period does not preclude acknowledging that in other times and places ascriptive group consciousness may be overwhelmed by class consciousness deriving from economic inequality. For contingent historical reasons—the massive influx of immigrants in the 1840s and '50s and the antislavery movement's challenge to the social and economic subordination of four million blacks—Americans of the mid nineteenth century may have been more likely to embrace racial, ethnic, or religious identities than those that derived from their relationship to the market or means of production.

Explaining northern sectionalism and southern separatism does not quite account for the coming of the war. What remains somewhat mysterious is why the people of the North were willing to fight and die to preserve the Union. This final link in the chain of Civil War causation is just beginning to receive the attention that it deserves. Southern independence would have posed no obvious threat to the spread of the free labor system to the existing federal territories, and it should have put to rest any fears that the slave states would dominate the central government. One explanation that has been offered recently for the North's adamant Unionism is that it reflected a deep popular commitment to law and order under the Constitution and more specifically a fear of total national disintegration through further secessions if the southern states were allowed to depart in peace.²⁵ Recent studies of the correspondence of northern soldiers show that a large proportion of them had a strong commitment to Unionist ideology and a surprisingly sophisticated understanding of it. Like President Lincoln, many of them recognized that tolerating secession was incompatible with the survival of a democratic republic and that the emancipation of the slaves might prove essential to the preservation of the Union. At the root of this devotion to the northern cause, according to one historian, was a tendency to identify loyalty to the Union

with the family obligations and values that were at the core of mid-nineteenth-century American culture.²⁶

In an effort to account for the determination of those in power to preserve the Union at all costs, another recent historian has argued that a breakup of the Union was intolerable to northern elites because it would have threatened the alliance of western family farmers and eastern industrialists that formed the backbone of the Republican party and the new “political economy” that was emerging in the North. Without a shared enmity to the South, this coalition would allegedly have broken up.²⁷ Like most other explanations based on “political economy” this one is plausible but is difficult to substantiate—one would be hard-pressed to find the spokesmen for the economic interests in question calculating in this fashion. Their pronouncements on the crisis suggest that they believed, rightly or wrongly, that harmony between industry and commercially oriented yeoman agriculture was natural and inevitable.

If historians are generally agreed that slavery was at the root of the antebellum sectional crisis (if not on the question of whether sectional differences on the future of slavery provide a sufficient explanation for the war itself), they also agree that emancipation was a war measure that could not have occurred under any other conceivable circumstances at this stage of American history. No effort to belittle the direct impact of the Civil War can get around the fact that it altered the status of African-Americans in a fundamental way.

The other obvious and immediate effect of the war was that it put down secession and thus preserved and strengthened the federal government. It was now clearly established that the Union was not a voluntary compact among the states and that the authority of the central government was supreme within its sphere. But historians have disagreed on precisely how that sphere was now defined and on how radically the constitutional basis of the American nation had been altered. Some have emphasized the degree of autonomy still possessed by the states and the continued weaknesses of the federal government—its lack of “administrative capacities”—in comparison to most European national states of the nineteenth century.²⁸ Others have stressed the assumption of new federal functions and prerogatives during and immediately after the war.²⁹

What needs more attention is the broader question of how much change of all kinds—political, social, economic, and cultural—can be attributed to the Civil War (beyond the elimination of chattel servitude and denial of the right of states to secede from the Union). How decisively and thoroughly did it alter American life in general? Was it really one of those great historical cataclysms that changes almost everything, or something less than that? Even when the issue is not explicitly addressed in these terms, much recent historical work on the postwar era provides at least a partial answer. But a review of this work reveals a striking lack of consensus and exposes an historiographical disagreement that badly needs analysis and clarification.

In the work of both Marxian and liberal practitioners of “political economy” one can often find a maximalist view of the effects of the war that follows logically from their analysis of its causes. The ancestry of this conception can be traced to

the claims that Charles and Mary Beard made in the 1920s that the Civil War was "the second American revolution," because it signified the triumph of the industrial interests represented by the North over the agricultural interests championed by the South.³⁰ But Beard's successors have given much more weight to slavery than to agrarianism as a source of southern distinctiveness and have tended to jettison Beard's direct economic determinism in favor of an analysis that concedes some autonomy to class-based cultures and ideologies. Contemporary advocates of the maximalist position do, however, share the Beardian view that the war altered the nation's political economy in a decisive way. Instead of two competing regional ruling classes—one basing its power and deriving its worldview from a pre-modern form of labor exploitation and the other from the modernizing "free labor system"—there was now only one. The ascendancy of an industrial, commercial, and financial bourgeoisie deriving its wealth and influence from its control of wage labor constitutes, in the opinion of most maximalists, the lasting effect of the Civil War crisis.³¹

Significant disagreements have developed, however, on the question of whether the southern way of life was immediately and radically transformed by the war and emancipation. For the neo-Marxian followers of Eugene Genovese, antebellum precapitalism was rooted in the fact that labor was owned rather than hired. It therefore followed that emancipation constituted a fundamental shift from a slave-based form of production to an orthodox capitalist one, with all the cultural and ideological reversals that such a basic change entailed.³² But other scholars influenced by Marxism have deemphasized slavery *per se* and have noted the survival of a plantation system based on variable forms of labor coercion well into the twentieth century. They have detected a revival or persistence of planter power in the postwar era, based on the quasislavery of sharecropping tenancy, that impeded the capitalistic modernization of the South, and to a lesser extent of the nation as a whole, for an extended period.³³

The most distinguished and influential liberal historians of the postwar South have tended to second the argument of those in the Marxian tradition that war and emancipation transformed the region's political economy in a decisive way. In an impressive body of work spanning more than four decades, C. Vann Woodward has contended vigorously against "continuistarians" who deny basic change. He has consistently maintained that the antebellum planter aristocracy was displaced during the postwar years by an emerging middle class with bourgeois values alien to the culture of the Old South.³⁴ Gavin Wright, the foremost economic historian of the New South, has conceptualized the shift as the abrupt transformation from an economy dominated by "labor lords" to one responsive to the rather different interests of "land lords." Although he acknowledges that there was considerable overlap in the personnel of prewar and postwar elites, Wright argues that a new relationship to the means of production altered the economic and political behavior of the South's dominant class in fundamental ways.³⁵ The dominant current view among analysts of the South's political economy is that the war made it significantly different from what it had been before, but dissent from this position persists.³⁶ Whether the change that occurred should be re-

garded as truly revolutionary or as an example of the kind of reform and readjustment that puts old wine in new bottles remains debatable.

The argument of maximalists that the northern victory contributed to the hegemony of industrial capitalism in the political economy of the nation as a whole seems well grounded. Maximalists find evidence for a new order in the enactment, during and immediately after the war, of economic legislation designed to promote capitalist development, such as high protective tariffs, subsidization of transcontinental railroads and other internal improvements, and contraction of the currency to favor creditors over debtors. It was also reflected in an increased willingness to use coercive state power to preserve the kind of internal order and domestic tranquillity that capitalist enterprise seemed to require. The use of federal troops to protect Republican governments in the South came to an end in 1877, but in that same year they were used to put down a national railroad strike, an action based on the wartime precedent of using the army to suppress labor actions that allegedly impeded the military effort.³⁷

A maximalist interpretation of the twists and turns of postwar politics cannot be as straightforward as it once was, because of the way that the Reconstruction era is now generally evaluated. For Charles A. Beard and earlier advocates of the "second American revolution" hypothesis, it was axiomatic that "Radical Reconstruction" was not about the rights of the freedpeople but was driven by the desire of northern capitalists to gain access to southern sources of wealth. But most contemporary historians of Reconstruction, including those who write from a political-economy perspective, are not as cynical as the Beardians were in evaluating the motives of the Radicals and are prepared to acknowledge that men like Thaddeus Stevens and Charles Sumner acted out of a kind of middle-class democratic idealism rather than at the behest of a hegemonic capitalist elite. If, however, the relatively brief period of federal activism on behalf of black rights (roughly 1867–72) is viewed as an afterglow of wartime zeal that increasingly lacked egalitarian conviction, or if the failure of Radical efforts to guarantee the civic equality of African-Americans is deemed virtually inevitable given the limited and inefficient means that the Republicans were willing or able to employ, it becomes possible to link the failure of the war-inspired attempt to reconstruct the South on the basis of black manhood suffrage to the decline of popular democratic politics that recent historians have detected toward the end of the century—and which to their way of thinking strengthened the position of dominant elites.³⁸ Requiring further study is the extent to which the chaos and failure of Reconstruction contributed to middle-class disenchantment with an electoral system based on universal manhood suffrage and nearly universal participation.

Virtually beyond challenge is the notion that the Republican party, once it harnessed or suppressed the radical-democratic impulse that to some extent expressed itself in congressional Reconstruction, became a party devoted primarily to the interests of the business community that was able to draw on its prestige as the Union-saving party to appeal to the patriotic and nationalistic sentiments of a majority of ordinary citizens in the northern states. Since the Republicans were nationally dominant in the entire period from 1865 to 1932,

the identification with American nationalism that it derived from its role in the sectional crisis was, or so it could be argued, of central importance in the development of the corporate capitalist hegemony that allegedly characterizes twentieth-century America.

The contrary or minimalist view of the effects of the Civil War is more often expressed indirectly or implicitly than systematically expounded. The idea that the Civil War revolutionized the nation is implicitly criticized in the mass of scholarship dealing with the mid-to-late-nineteenth-century America that manages to discuss major trends in American life and thought with scarcely a mention of the Civil War.

Most works on the social, intellectual, and cultural history of the nineteenth century devote little or no attention to the war. If they are seeking "watersheds," or periods of a decade or so when the climate of opinion shifted decisively, they may find them, as John Higham has done, in the 1850s and the 1890s, rather than in the 1860s.³⁹ Those who have attempted to quantify various trends in social history have often found that their graphs seem little affected by anything that happened between 1861 and 1865. Cultural historians dealing with subjects like the changing role of women and gender in the public sphere, as Mary Ryan has done, may find that wartime activities reflected or exemplified new tendencies, but they attribute little direct influence to the war itself.⁴⁰ Economic historians have generally found that the war actually retarded industrialization and economic growth, although not in a way that had a lasting effect.⁴¹

Students of voting behavior have sometimes found little difference in the kind of cultural concerns expressed by the electorate in 1830s and '40s and in the 1880s and '90s. Historians and political scientists dealing with party systems have not found a significant "realignment" during the war or in the immediate postwar years (although they do recognize that one took place in the 1850s). They note that the Democratic party survived the war and remained nationally competitive for the next thirty years. In an analysis of the intense and evenly balanced partisan competition that characterized American politics between 1838 and 1893, Joel Silbey finds that the war had little effect on the basic pattern.⁴² True Republican predominance was not achieved until the 1890s, when the GOP gained firm control of the Midwest for the first time. The combination of a solidly Republican Midwest and an even more solidly Democratic South brought an end to the era of close elections and huge voter turnouts.⁴³

Some historians who have written directly about the war and its impact have also minimized its long-term significance. Morton Keller has argued, for example, that the war-inspired centralization of power and authority in the federal government did not survive the 1870s. In that decade, he contends, there was a partial reversion to the *laissez-faire* and localism that had characterized the prewar polity.⁴⁴ In national economic policy, the protective tariff survived the '70s but direct subsidization of capitalist enterprise, such as land grants to railroads, came to an end.

Historians of black-white relations can hardly deny that the freeing of the slaves made a difference in the situation of southern African-Americans, but they

have also suggested that the “redemption” process that followed the brief Reconstruction episode caused a return, if not to slavery itself, at least to a status that in some ways was closer to servitude than to equal citizenship. Post-Reconstruction southern elites would never enjoy the kind of power over the federal government that their predecessors had exercised before the war, but a minimalist might stress the retarding effect that their reviving fortunes could still have on the modernizing and nationalizing projects of progressive northern elites.

Like most debates about change and continuity, the disagreement between the maximalists and the minimalists sometimes resembles a quarrel over whether a glass of water is half full or half empty. As is true of any cataclysmic event or apparent historical “watershed,” some things change and others remain the same. An effort to strike a reasonable balance between radical transformationists and what C. Vann Woodward calls the “continuistarians” would be sensitive to change but careful not to overstate it. Wartime emancipation, for example, did not make blacks the political and social equals of whites, but it did shift the struggle for black liberation to a new plane. As both Kenneth Stampp and Eric Foner have pointed out—in major synthetic works on Reconstruction that were published nearly a quarter century apart—it was of lasting and vital significance that the Fourteenth and Fifteenth Amendments to the Constitution, like the Thirteenth abolishing slavery, were enacted during a brief window of opportunity offered by the fracture of the Union. Constitutional provisions for equal black citizenship made the twentieth-century civil rights struggle possible by giving African-Americans a claim to full democratic rights.⁴⁵ I would add that absent these amendments it is likely that separatist nationalism and emigrationism, rather than some form of integrationism or egalitarian ethnic pluralism, would have dominated black political thought and action.

The minimalists are correct to point out that many of the new powers and responsibilities assumed by the federal government during and immediately after the war were later surrendered or allowed to fall into disuse. But the reversion to localism and *laissez-faire* was not total, and an important precedent had been set for the assumption of extraordinary powers during a war or a national emergency that could be represented as the equivalent of a war. Because of the Civil War experience, it was probably easier than it might otherwise have been to enact conscription and suppress allegedly disloyal activities during World War I, and the precedent of both wars facilitated the enormous growth of governmental responsibilities by the Roosevelt administration during the Great Depression. Americans have remained culturally averse to an activist central state in what they considered to be normal times, but they have been remarkably receptive to appeals for a vast expansion of government responsibilities when their leaders could persuade them that national survival or well-being was at stake.

It can also be argued with some cogency that the war contributed significantly to the basic transformation of American culture that historians committed to evolutionary models of social change have viewed as the inevitable result of modernization. Had there been no war the great transition from the values that reflected the agrarian individualism of the early republic to a recognizably modern

commitment to bureaucratic organization and technical efficiency—"rationalization" in the Weberian sense—would still have occurred sooner or later. But the war with its incentives for organized endeavor and the efficient use of resources probably accelerated the process. The notion that the Civil War was a catalyst for modernization was a major theme of Allan Nevins's magisterial *The War for the Union*, the last multivolume history of the war experience in all its dimensions and a work from which much can still be learned.⁴⁶

But the modernization paradigm encompasses only part of the story of the war's cultural and institutional impact. Its total effect on the dominant trends toward a modernized American society was more ambiguous than this viewpoint readily permits us to acknowledge. Although it contributed in certain ways to the emergence of modernist modes of thought and action, in other ways the Civil War crisis may have served to deflect the nation from the pattern of development to which it may have been predisposed by the stage of socioeconomic and technological development that it had reached by the 1860s and '70s.

A distinguishing feature of postwar electoral behavior was the extent to which wartime loyalties determined voter allegiances. "Vote the way you shot," was perhaps the most effective appeal that politicians could make. As a result, the most prowar sections of the North remained overwhelmingly Republican, and the South, after suppressing the votes of black and white Unionist Republicans, became solidly Democratic. This persistence of sectional politics into the twentieth century impeded national alignments based on class or shared economic interests that might otherwise have occurred to a greater extent. It was one factor, according to Lawrence Goodwyn, that doomed the Populist insurgency of the 1890s.⁴⁷ The failure of the United States to generate a party system that expressed the rational, material concerns of national classes or interest groups was not due entirely to the Civil War—ethnoreligious loyalties and racial attitudes had strongly influenced political behavior before the conflict. But the persistence of sectionalism made politicians more likely than ever to appeal to voters' ascriptive identities rather than to ideologies deriving from class position and material circumstances.

The war may also have helped to make the United States different in its pattern of state formation from most other industrializing societies by producing an irregular and premature welfare state that had the effect of impeding twentieth-century efforts to establish a permanent system of old age pensions and social insurance. In work that historians have not paid enough attention to, historical sociologists Theda Skocpol and Ann Shola Orloff have argued that the relatively generous veterans' pensions, that by the 1890s actually made the United States a statistical world leader in providing support for elderly or disabled men and their widows and orphans, became a drag on subsequent efforts to introduce permanent social security systems. The fact that the war pensions served the interests of one political party and only one section of the country helped to convince reformers that the American political system, still heavily dependent on patronage and susceptible to corruption, could not be trusted to administer national programs for the security of its citizens in a fair and efficient way.⁴⁸

Historian Stuart McConnell has exposed the regressive, antimodernizing ideology of the veterans' lobby in an important recent study of the Grand Army of the Republic.⁴⁹ The veterans, animated by a "millennialist republicanism," viewed their pensions as a just reward for the unique and nonrepeatable contributions they had made to saving the Union and perfecting the republic. This rationale not only precluded extending the system to noncombatants but even weakened the case for treating the veterans of future wars in a similarly generous fashion. Whether one emphasizes the response of political institutions, like Skocpol and Orloff, or follows McConnell in stressing the ideology that veterans had derived from their war experience, it seems clear that the impact of the soldiers' movement was to make it more difficult for the American state and political culture to confront some of the problems of an industrializing and urbanizing society.

The ongoing search for a nuanced middle ground between the minimalists and the maximalists on the question of how much difference the Civil War actually made is unlikely to come to any conclusions that will do violence to the popular perception that, next to the Revolution that brought the nation into being, the sectional crisis was the most significant and formative event in American history. The minimalist view that it was an anomalous interlude with little effect on the most significant processes at work in producing twentieth-century America remains unpersuasive. But equally dubious is the maximalist conception that the war made all the difference in the world and was in fact more important than the Revolution itself in determining the path of national development.

What is likely to emerge is the conclusion that the Civil War was not so much a second (and more decisive) American revolution as the completion of the first. It strengthened—but did not create—American nationalism. It moved African-Americans a step further toward equal citizenship, extending a process that began with gradual emancipation in the northern states during the post-Revolutionary era. It assisted the forces promoting capitalist development by shifting the balance of power from a primitive capitalism of ruthless accumulation and forced labor to a more progressive capitalism based on technological innovation and wage labor, although it would take almost a century for the South to overcome its legacy of social and economic backwardness. It encouraged new patterns of thought and culture but did not obliterate older ones.

Such a balance might be used to support the view, recently restated eloquently by James McPherson, that the war was indeed "the second American revolution."⁵⁰ But in many ways the effects of the war (if not the quantity of blood that it spilled) are more analogous to those of the major reforms or "revolutions from above" that strengthened or consolidated potentially powerful nation-states elsewhere in the world at about the same time—such as the tsar's abolition of serfdom in Russia, the Meiji Restoration in Japan, and the unification of Germany and Italy under Bismarck and Cavour—than to a radical overturning of the social and political order on the model of what happened in France in the 1790s, Russia in 1917, and China in the late 1940s. The war resulted in an amended and reinterpreted Constitution but not a totally new one. I doubt if one could find

other cases that historians would generally agree to classify as socially and politically revolutionary that did not result, at the very least, in an unmistakably new and different charter of government. In 1850 the United States could have been described as a developing capitalist society with a representative, republican government and a racial qualification for full citizenship. The same general categorizations would have held true in 1900, although the meanings and functions of its capitalism, republicanism, and racism were not the same as they had been before the war. Significant change had certainly occurred, but I remain unconvinced that the transformation deserves to be called a revolution in any sense that would make it comparable to the events in world history that clearly deserve such a designation.

NOTES

1. Among the many recent works that find a regression from democratic values and practices during the late nineteenth century are Alan Trachtenberg, *The Incorporation of America: Culture and Society in the Gilded Age* (New York, 1982), and Michael McGerr, *The Decline of Popular Politics: The American North, 1865–1928* (New York, 1986). The new political system that was taking shape around the turn of the century is well portrayed in Richard L. McCormick, *From Realignment to Reform in New York State, 1893–1910* (Chapel Hill, 1981).

2. See such studies as Sean Wilentz, *Chants Democratic: New York and the Rise of the Working Class, 1788–1850* (New York, 1986); Alan Dawley, *Class and Community: The Industrial Revolution in Lynn* (Cambridge, Mass., 1976); Charles Sellers, *The Market Revolution: Jacksonian America, 1815–1846* (New York, 1991); Leon Fink, *Workingmen's Democracy: The Knights of Labor and American Politics* (Urbana, 1983). David Roediger takes note of the racial qualification in the tradition of artisan republicanism in his *Wages of Whiteness: Race and the Making of the American Working Class* (London, 1991).

3. This is the guiding paradigm of Robert H. Wiebe's influential synthesis *The Search for Order, 1877–1920* (New York, 1967) and, in a more restricted sense, of Alfred Chandler's definitive work, *The Visible Hand: The Managerial Revolution in American Business* (Cambridge, Mass., 1977).

4. On the current state of western and frontier history, see Patricia Nelson Limerick, *The Legacy of Conquest: The Unbroken Past of the American West* (New York, 1987), and "Turnerians All: The Dream of a Helpful History in an Intelligible World," *American Historical Review* 100 (1995): 697–716.

5. The military historian Albert Castel, reviewing Matthew Gallman, *The North Fights the Civil War: The Home Front* (Chicago, 1994), in *Reviews in American History* 22 (December 1994): 597, accuses Gallman of present-mindedness for even raising the question of the war's political and social consequences. Gallman argued that the war had relatively little immediate effect on society and politics. Castel agrees with him but criticizes the attention the book gave to the issue on the grounds that there could not conceivably have been "a radical transformation of the existing social and political order," since the North's war aim was to preserve the Union, not to change it. It is a curious concept of history that allows for no changes other than those that were envisioned at the outset by the instigators of actions or policies that clearly had unforeseen consequences.

6. Thomas J. Pressly, *Americans Interpret Their Civil War* (Princeton, 1954). An updating of this work would be valuable.

7. Among the most important works that provide a "bottom up" view of developments surrounding the Civil War are Eugene D. Genovese, *Roll, Jordan, Roll: The World the Slaves Made* (New York, 1974); Herbert G. Gutman, *The Black Family in Slavery and Freedom* (New York, 1976); Lawrence W. Levine, *Black Culture and Black Consciousness: Afro-American Folk Thought from Slavery to Freedom* (New York, 1977); John Blassingame, *The Slave Community: Plantation Life in the Old South*, rev. ed. (Oxford, 1979); Elizabeth Fox-Genovese, *Within the Plantation Household: Black and White Women of the Old South* (Chapel Hill, 1988); Deborah White, *Ar'n't I a Woman: Females Slaves in the Plantation South* (New York, 1985); Reid Mitchell, *The Vacant Chair: The Northern Soldier Leaves Home* (New York, 1993); Leon F. Litwack, *Been in the Storm So Long: The Aftermath of Slavery* (New York, 1979); and Iver Bernstein, *The New York City Draft Riots: Their Significance for American Society and Politics in the Age of the Civil War* (New York, 1989). Eric Foner's general history, *Reconstruction: America's Unfinished Revolution, 1863–1877* (New York, 1988), departs from earlier syntheses by paying more attention to grassroots struggles in the South than to national politics. An intriguing effort to interpret the postwar reunion process in terms of gender is Nina Silber, *The Romance of Reunion: Northerners and the South, 1865–1900* (Chapel Hill, 1993).

8. Three recent winners are James McPherson's *The Battle Cry of Freedom: The Civil War Era* (New York, 1988), which presents a good deal of "bottom up" material but not to the neglect of leadership and decision making; Mark E. Neely, Jr., *The Fate of Liberty: Abraham Lincoln and Civil Liberties* (New York, 1991), which is a conventional political narrative of high quality; and Garry Wills, *Lincoln at Gettysburg: The Words That Remade America* (New York, 1992). It is indicative of the gap between popular and academic conceptions of history that these well-written and deeply researched works have been more successful with the educated general public than among the professoriat.

9. For a sense of this trend as it affects historians generally, see Lynn Hunt, ed., *The New Cultural History* (Berkeley, 1989). Several of the works cited above as examples of "bottom up" history also make use of anthropological perspectives. The explicit use of postmodernist theory is still rare in Civil War historiography, but one can find some of it in work that adopts an interdisciplinary American Studies perspective, such as Anne C. Rose, *Victorian America and the Civil War* (Cambridge, Eng., 1992).

10. In addition to works cited above, such as Rose, *Victorian America*, Silber, *Romance of Reunion*, Levine, *Black Culture*, and Mitchell, *Vacant Chair*, see also Earl J. Hess's effort to study wartime rhetoric in terms of the discourse of "republicanism"—*Liberty, Virtue, and Progress: Northerners and Their War for the Union* (New York, 1988).

11. In addition to the works of Genovese and Foner, which are cited elsewhere, see Barbara Jeanne Fields, *Slavery and Freedom on the Middle Ground: Maryland in the Nineteenth Century* (New Haven, 1985). Class analysis became central to the study of nineteenth-century African-American communities in studies such as Thomas Holt, *Black over White: Negro Political Leadership in South Carolina during Reconstruction* (Urbana, 1977), and Nell Irvin Painter, *Exodusters: Black Migration to Kansas after Reconstruction* (New York, 1977).

12. For a further discussion of these issues and references to some of the relevant works, see George M. Fredrickson, *The Arrogance of Race: Historical Perspectives on Slavery, Racism, and Social Inequality* (Middletown, Conn., 1988). Among the historians who adopt an essentially Weberian approach to black-white relations in the South is William J. Harris. See his *Plain Folk and Gentry in a Slave Society: White Liberty and Black Slavery in Augusta's Hinterlands* (Middletown, Conn., 1985).

13. See especially David M. Potter, *The Impending Crisis of the South, 1848–1861* (New York, 1976), pp. 30–50; idem, “The Literature on the Background of the Civil War,” in *The South and the Sectional Conflict* (Baton Rouge, 1968), pp. 87–150; and Kenneth M. Stampp, “The Irrepressible Conflict,” in *The Imperiled Union: Essays on the Background of the Civil War* (New York, 1980), pp. 192–245.

14. Eugene Genovese has not actually written an essay on the causes of the Civil War, but his views can be inferred from his works on slavery and slaveholders, especially *The World the Slaveholders Made: Two Essays in Interpretation* (New York, 1969). For Foner’s perspective, see his essays “The Causes of the American Civil War: Recent Interpretations and New Directions” and “Politics, Ideology, and the Origins of the Civil War,” in *Politics and Ideology in the Age of the Civil War* (New York, 1980), pp. 15–53.

15. Potter, *Impending Crisis*, pp. 42–43.

16. Quoted in Stampp, *The Imperiled Union*, pp. 193.

17. The revisionist view was summed up in Avery Craven, *The Coming of the Civil War* (New York, 1942). Potter’s earlier revisionism is evident in his *Lincoln and His Party in the Secession Crisis* (New Haven, 1942).

18. For differing interpretations of slavery that shared an implication that its persistence would have been morally untenable, see Kenneth M. Stampp, *The Peculiar Institution: Slavery in the Ante-Bellum South* (New York, 1956), and Stanley Elkins, *Slavery: A Problem in American Institutional and Intellectual Life* (Chicago, 1959).

19. Among the most important works in this tradition are Paul Kleppner, *The Third Electoral System: Parties, Voters, and Political Culture* (Chapel Hill, 1979); Michael Holt, *The Political Crisis of the 1850s* (New York, 1978); and Joel Silbey, *The Partisan Imperative: The Dynamics of American Politics before the Civil War* (New York, 1985).

20. Eric Foner, *Free Soil, Free Labor, Free Men: The Ideology of the Republican Party before the Civil War* (New York, 1970).

21. William E. Gienapp, *The Origins of the Republican Party, 1852–1856* (New York, 1987).

22. See various works of Eugene Genovese, especially *The World the Slaveholders Made* and *The Fruits of Merchant Capital: Slavery and Bourgeois Property in the Rise and Expansion of Capitalism*, coauthored by Elizabeth Fox-Genovese (New York, 1983).

23. James Oakes, *Slavery and Freedom: An Interpretation of the Old South* (New York, 1990), and *The Ruling Race: A History of American Slaveholders* (Knopf, 1982).

24. The best general statement of this argument is still Potter, *Impending Crisis*, chap. 17. For its application to one state, see also Steven A. Channing, *A Crisis of Fear: Secession in South Carolina* (New York, 1970).

25. Philip Shaw Paludan, “A People’s Contest”: *The Union and the Civil War, 1861–1865* (New York, 1988), chap. 1.

26. See James M. McPherson, *What They Fought For, 1861–1865* (Baton Rouge, 1994), on the role of ideology, and Mitchell, *Vacant Chair*, on the significance of familism.

27. Richard Franklin Bensel, *Yankee Leviathan: The Origins of Central State Authority in America, 1859–1877* (Cambridge, Eng., 1990), p. 93.

28. See Harold Hyman, *A More Perfect Union: The Impact of the Civil War and Reconstruction on the Constitution* (New York, 1973), and Stephen Skowronek, *Building an American State: The Expansion of National Administrative Capacities, 1877–1920* (Cambridge, Eng., 1982), pp. 29–31 and *passim*.

29. For a strong statement of this position, see David Montgomery, *Beyond Equality: Labor and the Radical Republicans, 1862–1872* (New York, 1967), pp. 46–48.

30. See Charles A. and Mary R. Beard, *The Rise of American Civilization*, 2 vols. (New York, 1927), II, 53–54.

31. See the previously cited works by Genovese, Foner, Bensel, and Montgomery, among others.

32. Barbara Fields, in *Slavery and Freedom*, applies such a maximalist understanding of the changes induced by emancipation to the state of Maryland. James Roark examines the behavior and consciousness of the planter class from a roughly similar perspective in *Masters without Slaves: Southern Planters in the Civil War and Reconstruction* (New York, 1977).

33. Prime examples of scholarship that stresses planter or plantation persistence are Jay R. Mandle, *Not Slave, Not Free: The African American Economic Experience since the Civil War* (Durham, 1992), and Jonathan M. Wiener, *Social Origins of the New South: Alabama, 1865–1885* (Baton Rouge, 1978).

34. C. Vann Woodward set forth his basic interpretation in *Origins of the New South, 1877–1913* (Baton Rouge, 1951) and has reaffirmed it in *Looking Back: The Perils of Writing History* (Baton Rouge, 1986).

35. Gavin Wright, *Old South, New South: The Revolutions in the Southern Economy since the Civil War* (New York, 1986).

36. See, for example, Carl H. Moneyhon, *The Impact of the Civil War and Reconstruction in Arkansas: Persistence in the Midst of Ruin* (Baton Rouge, 1994).

37. See Leonard P. Curry, *Blueprint for Modern America: Non-Military Legislation of the Civil War Congress* (Nashville, 1968), and Grace Palladino, *Another Civil War: Labor, Capital, and the State in the Anthracite Regions of Pennsylvania, 1840–1868* (Urbana, 1990).

38. Michael McGerr describes the postwar tendency in *The Decline of Popular Politics* but makes no effort to trace the roots of the transformation of political culture to the war and Reconstruction.

39. John Higham, *From Boundlessness to Consolidation: The Transformation of American Culture, 1848–1860* (Ann Arbor, 1969), and “The Reorientation of American Culture in the 1890s,” in *Writing American History: Essays on Modern Scholarship* (Bloomington, 1970), 73–102. An exception to the prevailing tendency to deemphasize the effect of the Civil War on intellectual history is George M. Fredrickson, *The Inner Civil War: Northern Intellectuals and the Crisis of the Union*, rev. ed. (Urbana, 1993; orig. pub. 1965).

40. See Mary Ryan, *Women in Public: Between Banners and Ballots, 1825–1880* (Baltimore, 1990).

41. Roger L. Ransom, an economic historian who has written a general study of the Civil War era, concludes that “the balance of scholarly opinion is that, taken on its broadest level, the direct economic impact of the war was to *retard* economic growth in the industrial sector, but probably to *stimulate* growth of agriculture outside the South. If we include the South, the net gain in agriculture was far less, and perhaps disappears altogether.” He goes on, however, to acknowledge some important indirect effects. See *Conflict and Compromise: The Political Economy of Slavery, Emancipation, and the American Civil War* (Cambridge, Eng., 1989), p. 264 and passim.

42. See Joel H. Silbey, *A Respectable Minority: The Democratic Party in the Civil War Era, 1860–1868* (New York, 1977), and *The American Political Nation, 1838–1893* (Stanford, Calif., 1991).

43. See Richard J. Jensen, *The Winning of the Midwest: Social and Political Conflict, 1888–1896* (Chicago, 1971).

44. Morton Keller, *Affairs of State: Public Life in Late Nineteenth Century America* (Cambridge, Mass., 1977).

45. Kenneth M. Stampp, *The Era of Reconstruction, 1865–1877* (New York, 1965), pp. 214–15; and Foner, *Reconstruction*, pp. 611–12.
46. Allan Nevins, *The War for the Union*, 4 vols. (New York, 1959–71).
47. Lawrence Goodwyn, *Democratic Promise: The Populist Movement in America* (New York, 1976), pp. 4–24.
48. Theda Skocpol, *Protecting Soldiers and Mothers: The Political Origins of Social Policy in the United States* (Cambridge, Mass., 1992), pp. 102–51; Ann Shola Orloff, *The Politics of Pensions: A Comparative Analysis of Britain, Canada, and the United States* (Madison, 1993), pp. 230–39.
49. Stuart McConnell, *Glorious Contentment: The Grand Army of the Republic, 1865–1900* (Chapel Hill, 1992).
50. James M. McPherson, *Abraham Lincoln and the Second American Revolution* (New York, 1990).

Americans and the Writing of Twentieth-Century United States History

JAMES T. PATTERSON

HISTORIANS of the United States have commented often—usually with asperity—about the enormous quantity of publications in many fields of research. Even the most industrious scholars, they complain, have time to read only small portions of the vast output that appears every year. That is surely the case concerning writing about twentieth-century United States history, a rich area indeed of historical research and publication. By my count, some 800 of the 1,400 articles identified in a recent comprehensive list compiled by the *The Journal of American History* are concerned with aspects of the United States since 1900.¹

Most of these articles tend to be highly focused efforts that interest relatively few readers. Still, the numbers indicate that a great many scholars teach and write in the area of twentieth-century American history. Moreover, the field is undeniably popular among the reading public and among college and university students in the United States. Enrollments in widely offered courses dealing with recent American history—and in the twentieth-century segments of more specialized courses (such as diplomatic history, intellectual history, and urban history)—have long been large at universities in the United States. Student interest in American history from World War II to the very recent past seems especially intense.²

Given the popularity of twentieth-century American history, one might expect academic practitioners of it to feel bullish about the present and the future of the field. Many are indeed upbeat, pleased with both the quality of scholarship and with the range of research, which has explored subjects, especially in social and cultural history, that had often been slighted by earlier generations of historians. Other scholars, however, tend to be uneasy. They lament first of all that histories and biographies by popularizers are attracting wide readership while more analytical academic work gathers dust on the shelves. They worry also that they participate in a large, amorphous, and in some ways fractured field—one that some time ago lost faith in a primarily political narrative that had given coherence to research, publication, and university courses. History-writing about twentieth-century America, they add, is less a field than a thicket. This observation has much to commend it. Scholarship concerning the United States since 1900 reflects the rapid growth and specialization of the historical profession as well as

the splintering of modern American society generally—a splintering that has encouraged a focus on ethnic distinctiveness, cultures of “resistance,” and identity politics. Each group, it seems, has its own historian.

The irony of this situation is that as the United States has become more centralized by mass communications and political and economic forces, the vision that has emerged from many of our academically based studies of twentieth-century developments—especially from those that dwell on social and cultural change—is of a nation that stubbornly resists homogenization. Unsettled by these historiographical trends, some political, economic, and diplomatic historians have complained that scholars pay too little attention to the powerful forces—international as well as national—that affect all of us, and too much—maybe far too much—to the ways in which we are divided.

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There is no doubting the appeal to the so-called general reading public of sweeping narratives and political biographies, many of them written by nonacademic historians, concerning the recent American past.³ Among those that have sold well in the past decade are Stephen Ambrose’s fast-paced account of the D-Day invasion, David Garrow’s biography of Martin Luther King, Jr., Taylor Branch’s epic treatment of the civil rights movement between 1954 and 1963, David McCullough’s biography of Harry Truman, David Halberstam’s history of the 1950s, Nicholas Lemann’s narrative of African-American migration from Mississippi to Chicago (and back), and Doris Kearns Goodwin’s book on the Roosevelts in the White House during World War II.⁴ Three of these books—by Garrow, McCullough, and Goodwin—have won Pulitzer prizes. The personal lives of presidents, generals, and other high-level actors on the national scene seem to have an especially timeless appeal to the American public.⁵

Why so much interest in histories such as these? One is both obvious and long-standing: people tend to be fascinated by narratives of exciting happenings—especially in politics, battles, and statecraft—that have taken place within their own lifetimes. Readers seem to hope that such narratives will offer an “inside story” of events—and of personalities—that they remember but know only a part of. This fascination, while discernible among readers around the world, may be particularly strong in the United States, which has had an uncommonly brief history; for most Americans, there is no *longue durée* to explore. As Denis Brogan pointed out more than forty years ago in speculating on differences between English and American approaches to history, educated English people tend to have a “deep, reverential sense of unity with a remote past.” Not so, he noted, in the United States. “The American is willing to look at the past, to display curiosity.” But “the past with which he is really connected is so short that history is either purely antiquarian or genealogical, or is in spirit modern; how did we get this way in 1954?”⁶

Although Brogan was not critical of such attitudes, others have complained about the tendency of Americans to approach history from a utilitarian perspec-

tive. This may indeed be an especially strong tendency in the United States, whose forward-looking people have often held great expectations about the “relevance” of history to the present and the future. James Harvey Robinson and other “New Historians”—many of them caught up in the reform spirit of the Progressive era after 1900—had such notions in mind.⁷ So did some of America’s leading intellectuals at the time. “A knowledge of the past and of its heritage,” John Dewey wrote in 1916, “is of great significance when it enters into the present, but not otherwise.” He added, “the mistake of making the records and remains of the past the main material of education is that it cuts the vital connection of present and past, and tends to make the past a rival of the present and the present a more or less futile imitation of the past.”⁸ Taking Dewey literally, educationists tried to turn history into civics courses and civics courses into propaganda.⁹ Other Americans of utilitarian temper, including an occasional policy maker, have imagined that “history” can provide “lessons” to help in steering the ship of state.¹⁰ In these and others ways the presentist, utilitarian tenor of American life and thought may heighten the appeal of twentieth-century history in the United States, especially military, diplomatic, and political history of World War II and the more recent past.

Presentism, at any rate, is surely and perhaps inevitably a salient aspect of much writing about twentieth-century United States life. The recent past, after all, hardly features unfamiliar or exotic terrain, and it is difficult if not impossible for historians who explore it to approach it anthropologically—as an “other” world that is markedly different from the society in which they live. On the contrary, emotional contemporary issues, notably those that concern race, ethnicity, and gender, powerfully affect historians as they study the not-very-distant sources of present-day problems.¹¹

The sheer quantity of source materials further explains the amount of writing about twentieth-century American history. To be sure, the proliferation of sources is hardly an unmixed blessing. Popular historians as well as scholars have been tempted to rely heavily on oral histories, both because such sources stand ready for use in the reconstruction of the recent American past and because they can help us to capture vividly the agency of ordinary people. Yet exploitation of personal recollections raises large questions of accuracy and verifiability. Moreover, the enormous amount of source material dealing with the recent past presents especially formidable problems of selectivity for the modern historian: many scholarly monographs dealing with twentieth-century United States history, as if despairing at the task of separating the wheat from the chaff, suffer from excessive detail. That is one large reason why they gather dust.¹² Finally, of course, many documents remain closed to researchers; this is a cause of special frustration among political and diplomatic historians.¹³ Still, the abundance of source materials, which include not only letters, official documents, and an enormous array of newspapers and magazines, but also film, television, and recordings, has surely encouraged researchers, thereby attracting many people to write and teach in modern United States history.

For some in the general reading public, modern American history has probably had yet one further appeal: the story of the United States in the twentieth century—at least to the mid-1960s—has been a peculiarly happy one. The United States has been spared the invasions, bloodletting, and famines that have blighted the modern histories of so many other nations since 1914. It has never feared for its survival—or even for the stability of its major institutions. For all its racial and ethnic divisions, it has featured a degree of equality and freedom enjoyed by few other nations. Its people not only managed to preserve democratic institutions through the perilous years of the Great Depression but also assumed major roles in winning two world wars. Following World War II (the “Good War”) the United States took unprecedentedly large steps toward establishing legal rights for African-Americans and other minorities. Surviving the excesses of McCarthyism, it witnessed substantial advances in the protection of civil liberties. The story of its economy from 1900 to 1970—the Great Depression excepted—was mainly one of fantastic progress.¹⁴

Given America’s relatively happy modern history, it is hardly surprising that many popular books have dwelt on World War II, the 1940s and 1950s, and early 1960s—peak years of American power and progress. Historians who wrote at the time—an era of self-congratulatory feeling in the United States—tended to celebrate the stability of America’s institutions, to extol its courageous role in the world wars and the Cold War, and to imagine that social “consensus” was blurring age-old divisions of race, class, ethnicity, and religion in the nation. By the early 1960s, many scholars—historians among them—cherished extraordinarily high expectations about the capacity of the nation to lead the “Free World” toward unprecedented levels of stability and happiness.¹⁵

Thereafter, a host of sobering events—the splintering of the civil rights movement, the rise of racial, class, and religious backlash, the war in Vietnam, the constitutional crisis of Watergate, the stagnation of the economy—induced historians (especially politically engaged academics) to offer much more jaundiced accounts of modern American life.¹⁶ Still, it seems fair to say that many so-called general readers have resisted such critical approaches. Seeking more positive, patriotic accounts, they have found much to cherish as they read about recent American history, especially narratives that focus on the years between 1940 and 1965.

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A look at the bulk of more academically based scholarship by historians about twentieth-century America provides a considerably less celebratory and more complex picture. Especially since the 1960s, the work of scholars in the field has tended to feature a seemingly bewildering outpouring of specialized histories, which in turn expose two major trends: the eclipse of state-centered political history and the explosion of writing in other subfields of the discipline. Both these trends reflect developments in the historical profession in general. They also reflect the ever sharper divisions of contemporary American society.

To be sure, there has long been a rough consensus among twentieth-century American historians on some matters. From the 1940s—when academic writing about the twentieth century began to flourish—to the present, most scholars and teachers specializing in recent American history have seemed to be relatively comfortable using the turn of the century as a periodizing point for the courses they teach and the texts that they assign to explore the years to follow.¹⁷ This is because the decade between 1896 and 1905 has generally seemed pivotal in many ways. During these years the frightening depression of the mid-1890s came to an end; Populism, a vital agrarian movement and third party, collapsed; a “great merger movement” centralized the corporate world; the Republican party, especially strong in the urban-industrial Northeast and Midwest, rode to a dominance in national politics that it maintained until 1930; “muckrakers,” exploiting a new mass-circulation journalism, energetically attacked malfeasance in American politics and business; intellectuals like William James, Thorstein Veblen, Charles Beard, and Dewey led a revolt against older formalisms; improved communications facilitated the rapid rise of national organizations, complex bureaucracies, and professional disciplines; lab-based science transformed teaching and research in medicine and other fields; a more hopeful “Progressive era” of social and political reform started to take shape; a modern style of politics, featuring direct primaries, declining partisanship, and the proliferation of powerful interest groups, asserted itself; a national administrative state began to spring into life; and the United States emerged as a major power on the international scene.¹⁸ A few years later, many of the major characteristics of a mass consumer society—notably automobiles, motion pictures, advertising and public relations, and commercialized sports—were becoming central to the culture.

Since the 1950s the majority of scholars writing twentieth-century United States history have seemed fairly content also with another periodizing point: the era of World War II. It was in these years that earlier trends—the rise of industrial labor unions, the triumph of agribusiness, the emergence of a Democratic political coalition, above all the expansion of the state and of interest group politics—became more solidly established in American life.¹⁹ Keynesian understandings of economic policy as well as a rights-based liberalism began slowly to evolve.²⁰ The years surrounding World War II also witnessed creation of the Manhattan Project and construction of the Pentagon, symbolizing the extraordinary power of military-industrial collaboration that persisted after 1945. And of course World War II, followed immediately by the rise of the Cold War, greatly expanded America’s involvement with the rest of the world. Reflecting the domestic and international impact of the war, many courses and textbooks divide twentieth-century United States history somewhere around 1945.²¹

Otherwise, however, scholars working in twentieth-century United States history do not find much consensus about periodization or other major aspects of the era. Some, indeed, seem uncertain and even a little defensive about what they are doing. In general, the academic field of recent United States history, while huge in the quantity of writing produced, has not been one of the most celebrated

research areas during the past twenty-five years. Other fields, notably research by “new” social historians specializing in the eighteenth and nineteenth centuries, have seemed more attractive to young scholars.²²

There are many reasons for this uncertain state of mind among academic practitioners of modern American history. One is the extraordinary pace of change and the great complexity of American life since 1900—and especially since 1945. How is it possible to know what is important and what is not? A second is the relative lack of historical perspective available concerning the very recent past: as a rule, academic historians are uncomfortable trying to make sense of events close to their own lifetimes. In part for this reason, many appear willing to accept an informal “twenty-year rule” that leaves study of the most recent past to journalists, sociologists, and political scientists, even though these writers may lack a strong background in history. (What they often do claim to have are general theories and large data sets, resources about which historians are often ambivalent.)²³ Most academic historians, moreover, do not believe that events in the past provide “lessons” for the present or for policy makers. Rejecting utilitarian notions of history, they lament the present-mindedness of the general reading public and of undergraduate students. Not a few consider “modern history” (especially since 1945) to be something of an oxymoron.²⁴

The main themes of scholarly writing about twentieth-century American history, moreover, tend to differ from those of earlier periods, therefore separating—in some ways isolating—academic historians of the period from other scholars. This separation stems in part from the aforementioned changes in American life one hundred years ago. Many historians of the eighteenth and nineteenth centuries, for instance, have dwelt on developments that seem less important in the twentieth-century world: the sources of the emergence of capitalism and market relationships, the role of the frontier and of westward expansion in the formation of American ideas and institutions, the rise and decline of sectionalism and states’ rights, the ongoing struggle for democracy and republicanism, the Civil War and Reconstruction, the impact of industrialization and urbanization. While some of these subjects—notably industrialization and urbanization—play significant roles in writings about the twentieth century, they are less central in them. Instead, scholars of recent United States history have tended to focus on other stories—the rise of America to world power, the expansion of the state, the influence of interest groups, the role of modern science and medicine, the explosion of professionalism and bureaucratization, the rush to the suburbs, the triumph of consumerism, the growth and stalemate of the civil rights revolution, the travails of feminism, the manifold meanings of mass popular culture. In these and other ways, the substance of many histories dealing with our century seems to spring, as if lacking deep roots, from the soil of a new world that developed in the late 1890s. While sensitive scholars writing about the twentieth century are concerned with the same big questions that engage other historians, they often focus on different problems and turn to very different sources.²⁵

Researchers interested in political trends, until the 1960s the most influential subfield of scholarship about modern American history, have been faced with

especially large changes in historiographical emphases since that time. In the 1950s, Richard Hofstadter, among others, began drawing heavily on writings in social psychology to undercut a once dominant form of “progressive” political history that in turn had offered a narrative framework used by many social, economic, and intellectual historians. This framework had tended to perceive past politics as a dualistic struggle between the forces of good (progressives, New Dealers, and reformers) and the forces of evil (corporate leaders and conservatives).²⁶ Hofstadter, however, rejoined that many “reformers” (especially in the “Progressive” era before 1920) had been deeply concerned about their social status amid the sweep of industrialization, urbanization, and mass immigration. They looked backward—to protect older (mostly Protestant, middle-class) values and ways of life—not forward to a new and multicultural society. Spearheading crusades for prohibition and immigration restriction, they displayed a profoundly moralistic, conservative, and ethnocentric view of the world.²⁷

Other historians in the late 1950s and 1960s criticized progressive versions of political and economic history from a somewhat different perspective, offering instead what became known as an “organizational synthesis” of twentieth-century America. Some of them drew heavily on theories of modernization that highlighted structural understandings of socioeconomic development. They also turned to sociology, especially to Weberian approaches stressing the rise of rational modes of decision making. Twentieth-century politics and social reform, these “organizational” historians insisted, have been highly complex, featuring shifting alliances of interest groups and ethnocultural rivalries. Significant local variations require careful research into specific communities. Scholars, these historians have emphasized, should pay less attention to progressive rhetoric and more to social structures, enduring ethnic and religious forces, and economic interests. A key development of modern American life, they have stressed, was the rise of large and powerful bureaucracies in the private sector, and a concomitant change in the nature of the political world—one that since 1900 has relied ever more heavily on “experts,” regulatory agencies, and administrative resources.²⁸

The organizational synthesis has for the most part been used by historians to describe economic and political change in America before 1945: much remains to be explored about large-scale bureaucracies, both public and private, in the postwar years. Moreover, it has not appealed to everyone. Heavy emphasis on bureaucratic structures—and more generally on the power of “modernization”—has seemed to some to be deterministic. Attention to large organizations has tended also to concentrate on the activities of elites, thereby (critics have argued) slighting the agency of ordinary people, the resistance of local groups to centralizing forces, and the role of dissenters on both the right and the left.²⁹ Still, the focus on significant structural changes, notably bureaucratization and economic centralization, has properly identified developments that are central to twentieth-century American life. Considerably more sophisticated than older dualistic approaches to political and economic history, the “organizational synthesis” has helped to replace an often episodic, personality-based study of politics and

economic development with an analytical approach that emphasizes the role of underlying structural forces. It has largely replaced the overly simplistic progressive narrative of the recent past.

Many political (and other) historians since the early 1960s have taken especially deadly aim against another older belief—that there was a specially progressive *direction* to American development. Already weak by 1940, this vision, like the related notion that the United States had an “exceptional” history, lost strength amid the bitter political and cultural battles that ravaged American society in the 1960s. Few scholars any longer see linear—or cyclical—progression of “reform” in twentieth-century life. They reject a “liberal narrative” linking the Progressive era, the New Deal, and the New Frontier–Great Society of the 1960s.³⁰ Instead, most historians writing about the current century are struck by the degree of complexity and diversity in America and by the power of conservatism, racism, and backlash as well as of liberalism and progress.³¹ There is today no clear consensus among academic historians about the direction or nature of American “reform” in the twentieth century.³²

The rejection of “progressive” versions of twentieth-century political history has not led to a widespread embrace of Marxist approaches to the period. Why this is so refers us to a much wider question—why has Marxist theory had relatively little influence on the academy?³³ But it clearly reflects the weakness of radical ideas and institutions in American society since the 1940s. The Communist and Socialist parties have virtually disappeared. Organized labor, once a source of hope for American social democrats, has become yet another interest group—and a weak one at that. And class as a category of analysis in recent years, an era of powerful civil rights and feminist activity, has attracted less interest among historians than have race and gender.³⁴ Like social scientists generally, historians of twentieth-century America have tended to be leery of many (not all) Marxist ideas, especially as they relate to political trends, and to employ a more pluralistic approach to economic and social change in the United States.³⁵

Still, there is no denying the critical, left-of-center stance of much academic history done since the mid-1960s about the recent American past. Radical and “New Left” approaches, while especially strong in the fields of labor and diplomatic history, have enjoyed a fair amount of broader appeal within the academy, mainly in the turbulent years of the 1960s and early 1970s.³⁶ A few works, indeed, claimed to discern significant conspiratorial activities by corporate leaders.³⁷ More commonly, historians sympathetic to the plight of the poor and the working classes have continued since the 1960s to highlight the particularly sharp inequality of American society, the sometimes impressive resistance of ordinary people to governmental and corporate centralization, and the persistence of republican-producerist ideas—in these respects, some of the main themes of social historians of earlier America have been carried forth into scholarship about the twentieth century.³⁸

Cultural historians in recent years have offered especially sharp critiques of twentieth-century American life. While only a few of these have openly embraced the presuppositions of Marxism or of postmodernist ideas, some identify “struc-

tures of domination,” “hegemonic” cultural forces, and paradigms and “discourses” that threaten to limit the agency of people.³⁹ Struck especially by the sweep of the consumer culture, they maintain that commercialization has run rampant in modern American life. Uniting many approaches of this sort are three important assumptions about twentieth-century American history: that consumer capitalism has assumed special power—psychological as well as economic—since 1900; that it has deeply affected—and damaged—the quality of American civilization; and that it has given rise to much that is notably modern and postmodern about American culture.

The attractions of this approach—it is too much to call it a “school” or a synthesis—have helped to make research in the area of popular culture—film, the mass media, advertising—perhaps the most dynamic subfield of scholarship concerning twentieth-century United States history. The researchers engaged in this enterprise, many of them in American Studies departments of leading universities, argue energetically among themselves about the extent to which the consumer culture has affected the citizenry—some emphasizing the stubbornly maintained agency and individuality of ordinary people, others lamenting the hegemony of commercialization and commodification. Most scholars in this broad field of inquiry, however, tend to offer a view of twentieth-century United States history that comes from the left and that is highly critical of long-range trends.⁴⁰

The growing appeal of cultural history reveals a larger historiographical trend since the 1960s—the tendency of younger scholars to distance themselves from a once dominant form of political history that had focused on the doings of federal government leaders and institutions. Coming of age amid the turmoil of dissent aroused by the civil rights movement, the Vietnam War, and Watergate, these younger scholars grew disenchanted with the state, which seemed to be a source of intrigue, corruption, and reaction. Much political and diplomatic history, they came to believe, was “old-fashioned”—the study of Elite and Powerful White Men. One manifestation of such disenchantment has been a turning away from research into two areas that had been very popular in the 1950s and early 1960s: the study of national politics in the Progressive and New Deal eras.⁴¹ Instead, younger historians have focused on the private lives of ordinary people and on “webs of cultural discourse,” thereby in some ways recalling the manifestos of the “New Historians” many years earlier.⁴² Devoting special attention to the postwar era, they have often identified with critics of American life. They have sought especially to write about outsiders—activists in the civil rights movement, spokespersons for a New Left, feminists, ethnic militants—and more generally to revive “history from the bottom up.”⁴³

The growing interest of scholars of twentieth-century American life in history from the bottom up is of course part of a much larger story: the extraordinary explosion of social history that has shaken the profession in the past thirty years. This explosion, to be sure, has most significantly rattled other fields of American scholarship, especially those dealing with the eighteenth and nineteenth centuries. It has not done quite as much to transform writing about the twentieth

century, where political and diplomatic history maintain some presence, albeit of a different sort from earlier years. The reason for this endurance is obvious: it is often impossible to leave the state—which has grown rapidly (especially since 1940)—out of the stories that we tell about the twentieth century. To do so, many scholars rightly insist, is to ignore the role of economic and political power—international as well as national—in the highly interconnected world of postwar America. The public irrevocably impinges on the private.⁴⁴

Moreover, political scientists—and to a lesser extent, sociologists—have recently been reaching out to historians in order to revive the fortunes of national political history.⁴⁵ This is in part because of sharp divisions within these social sciences since the 1960s: many political scientists in particular have rejected what they consider to be the arid model-building, behaviorism, and ahistoricism within their discipline. In 1990 they formed a Section on Politics and History within the American Political Science Association. By late 1992 the section had more than five-hundred dues-paying members.⁴⁶ Some of these scholars have joined historians in writing for *Social Science History*, *Studies in American Political Development* (1986–), and the *Journal of Policy History* (1989–), which publish articles that attempt to bridge gaps between history and the social sciences.

A few of these political scientists and sociologists have attracted particular attention among political historians by arguing that the state has played an important autonomous role in modern American life.⁴⁷ Turning to the history of social policy to document their case, they have focused on the growing power of state bureaucracies and institutions.⁴⁸ Their work has surely not convinced all historians that the state has in itself been the dominant player in policy making—many tend instead to emphasize the primary role of extragovernment forces, especially lobbies and social activists.⁴⁹ Changes in policy, these critics say, depend more on pressure from the bottom up than from the top down. Moreover, significant theoretical issues continue to separate historians from social scientists: the notion still persists that history deals in “facts” and that social “science” excels in theory.⁵⁰ Still, it is clear that political scientists, sociologists, and political historians dealing with twentieth-century developments are talking with one another more today than was the case fifteen or twenty years ago.

If, finally, we define twentieth-century “political” history in a broader sense—as the study of social movements seeking to expand the rights of citizenship—we can see that it is alive and well. Research into the civil rights movement—with which many scholars have identified deeply—has especially flourished, as has work on the New Left and feminism.⁵¹ While most of this writing has focused on the activists, not on the role of the state, some of it has necessarily concerned itself with the behavior of political institutions. For these reasons it is not entirely the case, as some political historians have tended to think, that social history is sweeping everything before it.⁵² Rather, there is now some evidence of a trend toward greater integration of social history from the bottom up and political history from the top down. Exploring links between the public and the private seems to be high on the agenda of many young scholars today.

Notwithstanding these efforts toward bridging gaps, there is no doubt that the rise of social history, among other developments, has placed many political historians—those who focus on the role of federal government officials—on the defensive. This sort of political history has either attenuated, as in the case of “top-down” studies of policy and elections, or become separated from other forms of historical writing, as in the case of diplomatic history.⁵³ The relative marginalization of research and writing in these fields—once central to narratives about twentieth-century United States history—is an especially noteworthy historiographical development. The old adage “History is past politics, and politics present history” long ago lost its appeal among younger historians interested in America in the twentieth century.⁵⁴

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As the rise of social history indicates, a good deal of recent academic writing about twentieth-century America has highlighted the flaws of civilization in the United States. It has paid great attention to the development of perceived problems of our own times: the power of a “military-industrial complex,” the hegemonic capacity of big business, the glaring limitations of the American welfare state, the persistence of racism, nativism, and sexism. Highly critical approaches such as these attest not only to the existence of real problems in the United States but also to the ways in which the post-World War II escalation of rights-consciousness in American culture—one of the the most compelling forces of our times—has expanded expectations about life. Americans are in many ways doing better, but nonetheless are feeling worse. Academic historians in the United States, most of them progressive in their politics, seem especially to reflect such feelings, and tend as a result to downplay the extraordinary technological, scientific, and political changes—many of them promoting freer, more comfortable lives for people—that have taken place in our lifetime.⁵⁵

If American scholars were to do more to compare historical developments in the United States with those in other nations, they might arrive at somewhat less gloomy perspectives.⁵⁶ Seen in the comparative context of the catastrophes and brutalities that have afflicted many other parts of the world since 1914, the history of the United States in the twentieth century might seem a little less grim than it is often portrayed. “Progress,” moreover, is not entirely a mirage or a myth in twentieth century America—witness the expansion of liberal public policies in such areas as civil rights and civil liberties. By and large, however, American historians who specialize in the twentieth century (as well as those who do not) have shied away from doing comparative studies, which involve formidable research agendas. Instead, they have tended to concentrate closely on trends in their own culture, paying little attention to major currents of European historiography.⁵⁷

To end on such critical notes, however, is to obscure a final important trend: the increasing diversity of historical writing on twentieth-century American life. Many scholars who undertake this work, which ranges far beyond political and

diplomatic history, do not identify themselves primarily as “twentieth-century American historians.” Instead, they tend to write as specialists in topical areas, such as cultural, social, economic, urban, or intellectual history, and they are more likely to teach these specialties than to offer politically centered surveys of the United States in the twentieth century.

Their output, which can only be summarized here, is impressive both in quality and quantity—the more so, it seems, as time passes. In recent years historians have been giving more than passing attention to many of the most powerful forces of twentieth-century American life, notably the impact of technology, business, science, and medicine.⁵⁸ Educational history, though most often written by professors of education, attracts a few historians.⁵⁹ The study of twentieth-century intellectual and religious history has also maintained some appeal, especially since the 1980s, when it became obvious that religious commitments remained powerful in American society. Much of this research, moreover, makes a special effort to contextualize developments, relating them to important institutional and social trends.⁶⁰ Interest in regional and environmental history, also reflecting contemporary societal concerns, has also begun to thrive—often (but not always) among historians who are uncomfortable with the organizational synthesis. Many of these works, too, attempt to link social, intellectual, and political developments.⁶¹ Labor history, while less central to scholarship on the late twentieth century than it is for earlier eras, continues to engage some scholars—a few of them focusing on the role of the state, others writing in the tradition of the new social history.⁶² And legal history, while most often tackled by journalists, lawyers, and political scientists, remains an active subfield within the historical profession.⁶³

Although the diversity of this scholarship has reflected the social fragmentation of our times, imaginative young historians continue to look for larger patterns. Lizabeth Cohen, for instance, has attempted with considerable success to bring cultural, labor, and (to a lesser extent) political history together in her book on Chicago industrial workers during the interwar period.⁶⁴ Steven Biel, exploring reactions to the sinking of the *Titanic*, has brought sophisticated cultural analysis to his subject.⁶⁵ Allan Brandt, among others, has looked at medical developments within the context of professionalization, cultural change, and political institutions: whiggish approaches to the history of science and medicine now seem very old-fashioned.⁶⁶ Carl Husemoller Nightingale has pulled together research into poverty, race relations, child-rearing patterns, and the consumer culture in order to offer a bold historical interpretation of postwar African-American life.⁶⁷ And James Goodman, another young scholar, has explored developments in race relations, gender conflict, political change, and the law to provide a study (postmodern in some of its narrative strategies) of the contentious issues surrounding the “Scottsboro boys” charged with rape in Alabama in 1931.⁶⁸

Scholars looking at twentieth-century gender relations, one of the most rapidly growing academic fields, have come forth with a number of studies connecting women’s, political, and social history, especially in work on social reform and social welfare. Indeed, the role that women have played in early and mid-

twentieth-century politics—and more broadly in the supposedly masculine “public sphere”—has received great attention in the last few years, helping to revive historical interest in some aspects of reform activity during the Progressive era and the 1930s.⁶⁹ These and other historians make it clear that scholars are continuing to step beyond the boundaries of specialization and to look for ways to tell more integrated stories about the recent past.

It is doubtful, however, that academic historians of twentieth-century America will come to agreement about any single synthesis of the era (or, for that matter, of any era.) We live, after all, in an age which highlights relativistic and personal ways of looking at things and which makes us acutely aware of group conflicts—of class, race, region, religion, gender, and ethnicity. Much that we write about is therefore decentered—unavoidably so. Some popular histories, to be sure, will continue to tell more heroic stories, but scholars are unlikely to return to interpretations based on simple dualisms in life, to describe America as an “exceptional” land of progress, or to employ schemes of periodization that rely primarily on political epiphenomena, important though those often are. While Americans surely display distinctive values and styles of life, we are also a polyglot and contentious people. For these reasons we cannot resurrect such a thing as an “American character.” Diversity and disagreement in historical scholarship about the United States in the twentieth century, as in modern life, seem destined to continue.⁷⁰

NOTES

1. “Recent Scholarship,” *Journal of American History* 81 (September 1994): 847–917. According to my rough categorization, articles in intellectual/cultural history and in international relations are most numerous, followed by articles dealing with women’s, African-American, and political history. For an annotated listing of 873 books and articles in United States history concerned with the years between 1920 to 1993, see American Historical Association, “United States History since 1920,” *Guide to Historical Literature* (New York, 1995), 1459–1503.

2. Modern United States history also appears to be popular in parts of western Europe—both among researchers and students. See Willi Paul Adams, “On the Significance of Frontiers in Writing American History in Germany,” *Journal of American History* 79 (September 1992): 463–71; Tony Badger, “Confessions of a British Americanist,” *ibid.*, 515–23; and Maurizio Vaudagna, “The American Historian in Continental Europe: An Italian Perspective,” *ibid.*, 532–42.

3. For comments on the place of nonacademic historical writing in contemporary America, see Nicholas Lemann, “History Solo: Non-academic Historians,” *American Historical Review* 100 (June 1995): 788–98.

4. Stephen Ambrose, *D-Day, June 6, 1944: The Climactic Battle of World War II* (New York, 1994); David Garrow, *Bearing the Cross: Martin Luther King, Jr., and the Southern Christian Leadership Conference* (New York, 1986); Taylor Branch, *Parting the Waters: America in the King Years, 1954–1963* (New York, 1988); David McCullough, *Truman* (New York, 1992); David Halberstam, *The Fifties* (New York, 1993); Nicholas Lemann, *The Promised Land: The Great Black Migration and How It Changed America* (New York, 1991); Doris

Kearns Goodwin, *No Ordinary Time: Franklin and Eleanor Roosevelt: The Home Front in World War II* (New York, 1994). Garrow is a political scientist. Branch, Lemann, Halberstam, McCullough, and Goodwin are independent writers.

5. Television series, too, have tapped this interest. Among the shows that have appeared in recent years are series dealing with the civil rights movement ("Eyes on the Prize"), the Great Depression, and the War on Poverty. Programs on national affairs—World War II, presidents (especially Franklin Roosevelt, John Kennedy, and Lyndon Johnson)—have also appeared.

6. "The Writing of History: An English Authority Compares British and American Viewpoints," *American Heritage* 6 (December 1954): 70–72. An exception to this generalization concerns the American Civil War, about which writings (especially on military matters) abound.

7. James Harvey Robinson, *The New History* (New York, 1912).

8. John Dewey, *Democracy and Education* (New York, 1916), 88.

9. Frances Fitzgerald, *America Revised: History Schoolbooks in the Twentieth Century* (Boston, 1979), 174–79.

10. A recent example is Robert McNamara, *The Tragedy and Lessons of Vietnam* (New York, 1995). This quest for usable "lessons" has often been a futile effort. See Richard Neustadt and Ernest May, *Thinking in Time: The Uses of History for Decision Makers* (New York, 1986), for cogent examples.

11. See Alan Ehrenhalt, *The Lost City: The Forgotten Virtues of Community in America* (New York, 1995), for a lament about the loss of "community" in the United States since the 1950s.

12. Excessive detail is of course a problem of historical monographs in many fields, not just in recent American history.

13. Social historians also confront problems concerning sources. Manuscript census materials are generally closed to researchers for seventy years, thereby protecting privacy.

14. The contrast between this experience and the histories of other nations during the twentieth century (especially between 1914 and 1945) is highlighted in Eric Hobsbawm, *Age of Extremes: The Short Twentieth Century* (London, 1994). Also see James Patterson, *Grand Expectations: The United States, 1945–1974* (Oxford, 1996); and Michael Elliott, *The Day before Yesterday: Reconsidering America's Past, Rediscovering the Present* (New York, 1996).

15. For comments on these trends see John Higham, "The Cult of the American Consensus: Homogenizing Our History," *Commentary* 27 (February 1959): 93–100; Higham, "Beyond Consensus: The Historian as Moral Critic," *American Historical Review* 67 (April 1962): 609–25; Richard Pells, *The Liberal Mind in a Conservative Age: American Intellectuals in the 1940s and 1950s* (New York, 1985); David Ricci, *The Tragedy of Political Science: Politics, Scholarship, and Democracy* (New Haven, 1984); Robert Dahl, *A Preface to Democratic Theory* (Chicago, 1956); John Blum, *Liberty, Justice, Order: Essays on Past Politics* (New York, 1993), 4–6.

16. For trends in historiography after 1960 see John Higham, "Changing Paradigms: The Collapse of Consensus History," *Journal of American History* 76 (September 1989): 460–66; Higham, "The Future of American History," *Journal of American History* 80 (March 1994): 1289–1309; Michael McGerr, "The Price of the 'New Transnational History,'" *American Historical Review* 96 (October 1991): 1056–67; David Hollinger, "How Wide the Circle of the 'We'? American Intellectuals and the Problem of the Ethnos since World War II," *American Historical Review* 98 (April 1993): 317–37; Kenneth Cmiel, "History against Itself," *Journal of American History* 81 (December 1994): 1169–74; and Peter Novick, *That*

Noble Dream: The "Objectivity Question" and the American Historical Profession (New York, 1988), esp. 573–629.

17. For many European societies, 1914 is a more common break point than 1900.

18. Key sources concerning this era include Naomi Lamoreaux, *The Great Merger Movement in American Business, 1895–1904* (New York, 1985); Arthur Link and Richard McCormick, *Progressivism* (Arlington Heights, Ill., 1983); John Chambers, *The Tyranny of Change: America in the Progressive Era, 1900–1917* (New York, 1980); Richard McCormick, "The Discovery That Business Corrupts Politics: A Reappraisal of the Origins of Progressivism," *American Historical Review* 86 (April 1981): 247–74; Daniel Rodgers, "In Search of Progressivism," *Reviews in American History* 10 (December 1982): 113–32.

19. Brian Balogh, "Reorganizing the Organizational Synthesis: Federal-Professional Relations in Modern America," *Studies in American Political Development* 5 (spring 1991): 119–72, emphasizes the war years as the time when a strong "proministrative state" finally arose in America.

20. The best recent history of many of these developments is Alan Brinkley, *The End of Reform: New Deal Liberalism in Recession and War* (New York, 1995).

21. Some subfields of modern United States history, to be sure, employ other periodizing points. See Michael McGerr, "Political Style and Women's Power, 1830–1930," *Journal of American History* 77 (December 1990): 864–85, on cycles in feminism.

22. Alan Brinkley, "Writing the History of Contemporary America: Dilemmas and Challenges," *Daedalus* (winter 1984): 121–41. For overviews of various subfields of twentieth-century American history, see Richard McCormick, "Public Life in Industrial America, 1877–1917," Walter La Feber, "Liberty and Power: U.S. Diplomatic History, 1750–1945," Brinkley, "Prosperity, Depression, and War, 1920–1945," and William Chafe, "America since 1945," in Eric Foner, ed., *The New American History* (Philadelphia, 1990), 119–41, 271–90, 93–117, 143–60; and James Patterson, "United States History since 1920," in American Historical Association, *Guide to Historical Literature* (New York, 1995), 1453–57.

23. Among widely read books on post-World War II American politics and society—many of them used in history courses—are Thomas and Mary Edsall, *Chain Reaction: The Impact of Race, Rights, and Taxes on American Politics* (New York, 1992); E. J. Dionne, Jr., *Why Americans Hate Politics* (New York, 1991); Jonathan Rieder, *Canarsie: The Jews and Italians of Brooklyn against Liberalism* (Cambridge, 1985); Landon Jones, *Great Expectations: America and the Baby Boom Generation* (New York, 1980); J. Anthony Lukas, *Common Ground: A Turbulent Decade in the Lives of Three American Families* (New York, 1985); James Davison Hunter, *Culture Wars: The Struggle to Define America* (New York, 1991); Garry Wills, *The Kennedy Imprisonment: A Meditation on Power* (Boston, 1982); and Wills, *Nixon Agonistes: The Crisis of the Self-Made Man* (New York, 1970). The Edsalls, Dionne, Jones, and Lukas are journalists; Rieder and Hunter are sociologists.

24. Even so, it is probably true that scholarly openness to "contemporary history" is greater in the United States than in many other nations, especially those with self-perceived long histories. This is in part because of the aforementioned presentism of American culture and in part because graduate schools in many other nations continue to train relatively large numbers of young historians specializing in earlier periods. In these nations historians of the most recent eras thus tend to feel marginalized. See Maurizio Vaudagna, "American History at Home and Abroad," *Journal of American History* 81 (December 1994): 1157–68. "Contemporary" history also attracts considerable scholarly interest in Great Britain and in Germany.

25. For some areas of historical investigation, of course, the "turn of the century" has little if any meaning for periodization. These areas include the history of immigration and

ethnicity, of urbanization, of religious conflicts, and of technology. See John Bodnar, *The Transplanted: A History of Immigrants in Urban America* (Bloomington, Ind., 1985); and Samuel Hays, *City at the Point: Essays on the Social History of Pittsburgh* (Pittsburgh, 1989).

26. Several books in the prestigious New American Nation series, widely read by graduate students in the late 1950s and early 1960s, reflect this dualistic approach. Among them are George Mowry, *The Era of Theodore Roosevelt and the Birth of Modern America, 1900–1912* (New York, 1958); Arthur Link, *Woodrow Wilson and the Progressive Era, 1910–1917* (New York, 1954); and John Hicks, *Republican Ascendancy, 1921–1933* (New York, 1960). See especially Arthur Schlesinger, Jr., *The Age of Roosevelt*, 3 vols. (Boston, 1957–60).

27. Richard Hofstadter, *The American Political Tradition and the Men Who Made It* (New York, 1948); Hofstadter, *The Age of Reform: From Bryan to F.D.R.* (New York, 1955); and Hofstadter, *The Progressive Historians: Turner, Beard, Parrington* (New York, 1968). Also see Daniel Joseph Singal, "Beyond Consensus: Richard Hofstadter and American Historiography," *American Historical Review* 89 (October 1984): 976–1004.

28. Alfred Chandler, Jr., *The Visible Hand: The Managerial Revolution in American Business* (Cambridge, Mass., 1977); Samuel Hays, *The Response to Industrialism, 1885–1914* (Chicago, 1957); Robert Wiebe, *The Search for Order, 1877–1920* (New York, 1967); Louis Galambos, "The Emerging Organizational Synthesis in Modern American History," *Business History Review* 44 (autumn 1970): 279–90; Galambos, "Technology, Political Economy, and Professionalization: Central Themes of the Organizational Synthesis," *Business History Review* 57 (1983): 471 ff; Balogh, "Reorganizing the Organizational Synthesis." An early salvo fired at "old-fashioned" political history was Thomas Cochran, "The 'Presidential Synthesis' in American History," *American Historical Review* 53 (1948): 748–59. See also Lee Benson, *The Concept of Jacksonian Democracy: New York as a Test Case* (Princeton, 1961).

29. A criticism along these lines is Brinkley, "Writing the History of Contemporary America."

30. See three revealingly titled books: Steve Fraser and Gary Gerstle, eds., *The Rise and Fall of the New Deal Order, 1930–1980* (Princeton, 1989); Brinkley, *End of Reform*; and Allen Matusow, *The Unraveling of America: A History of Liberalism in the 1960s* (New York, 1984). See also David Burner, *Making Peace with the 60s* (Princeton, 1996), which focuses on the eclipse of liberalism in the 1960s.

31. A relevant exchange of views on aspects of this subject is Alan Brinkley, "The Problem of American Conservatism," *American Historical Review* 99 (April 1994): 409–29, and Leo Ribuffo, "Why Is There So Much Conservatism in the United States and Why Do So Few Historians Know Anything about It?" *ibid.*, 438–49. See also Godfrey Hodgson, *The World Turned Right Side Up: A History of the Conservative Ascendancy in America* (Boston, 1996); and Michael Kazin, *The Populist Persuasion: An American History* (New York, 1995). Scholars looking at the American Red scare of the 1940s and 1950s have also emphasized the weakness of liberal ideas and institutions. For excellent syntheses see Richard Fried, *Nightmare in Red: The McCarthy Era in Perspective* (New York, 1990); and Stephen Whitfield, *The Culture of the Cold War* (Baltimore, 1991).

32. David Thelen, "The Practice of American History," *Journal of American History* 81 (December 1994): 933–68. Some historians, however, refuse to despair. See William O'Neill, *American High: The Years of Confidence, 1945–1960* (New York, 1986); and John Diggins, *The Proud Decades: America in War and Peace, 1941–1960* (New York, 1988), for more positive accounts of postwar United States history. My own view resembles theirs; see below.

33. Ian Tyrrell, *The Absent Marx: Class Analysis and Liberal History in Twentieth Century America* (Westport, Conn., 1986).

34. An important exception is Christian Appy, *Working-Class War: America's Combat Soldiers and Vietnam* (Chapel Hill, N.C., 1993).

35. See David Harvey, *The Urbanization of Capital: Studies in the History and Theory of Capitalist Urbanization* (Baltimore, 1985), for sophisticated use of Marxist ideas about capitalist accumulation.

36. See Gabriel Kolko, *The Triumph of Conservatism: A Reinterpretation of American History, 1900–1916* (New York, 1963); James Weinstein, *The Corporate Ideal in the Liberal State, 1900–1918* (Boston, 1968); Barton Bernstein, "The New Deal: The Conservative Achievements of Liberal Reform," in Bernstein, ed., *Towards a New Past* (New York, 1968); and Howard Zinn, ed., *New Deal Thought* (New York, 1966), xv–xxxvi. See also the work in diplomatic history of William Appleman Williams, such as *The Tragedy of American Diplomacy* (New York, 1959) and *The Contours of American History* (New York, 1961).

37. Notably Kolko, *Triumph of Conservatism*.

38. Examples—most of them books on American history before World War II—include David Montgomery, *The Fall of the House of Labor: The Workplace, the State, and American Labor Activism, 1865–1925* (New York, 1987); Alan Brinkley, *Voices of Protest: Huey Long, Father Coughlin, and the Great Depression* (New York, 1982); Jacquelyn Dowd Hall et al., *Like a Family: The Making of a Southern Cotton Mill World* (Chapel Hill, N.C., 1987); Gary Gerstle, *Working Class Americanism: The Politics of Labor in an Industrial City, 1914–1960* (New York, 1989); and James Grossman, *Land of Hope: Chicago, Black Southerners, and the Great Migration* (Chicago, 1989).

39. For an identification and critique of such emphases, see John Diggins, "Language and History," *Reviews in American History* 17 (March 1989): 1–9.

40. Two collections of essays edited by Richard Wightman Fox and T. J. Jackson Lears, *The Culture of Consumption: Critical Essays in American History, 1880–1960* (New York, 1983) and *The Power of Culture: Critical Essays in American History* (Chicago, 1993), offer useful starting points into this now large literature. See also Richard Butsch, *For Fun and Profit* (Philadelphia, 1989); William Leach, *Land of Desire: Merchants, Power, and the Rise of a New American Culture* (New York, 1993); Warren Susman, *Culture as History: The Transformation of American Society in the Twentieth Century* (New York, 1984); and Lawrence Levine, "The Folklore of Industrial Society: Popular Culture and Its Audiences," *American Historical Review* 97 (December 1992): 1369–99, and responses. Other studies include George Lipsitz, *Time Passages: Collective Memory and American Popular Culture* (Minneapolis, 1990); and James Baughman, *The Republic of Mass Culture: Journalism, Filmmaking, and Broadcasting in America since 1941* (Baltimore, 1992).

41. See Mark Leff, "Revisioning U.S. Political History," *American Historical Review* 100 (June 1995): 829–53. See also the thorough bibliography by Anthony Badger, *The New Deal: The Depression Years, 1933–1940* (New York, 1989). It indicates that most studies of politics and policies concerning the New Deal era were published before 1970 and that approaches to the subject have not changed much since then.

42. For a provocative lament about this trend, written from a radical perspective, see Tony Judt, "A Clown in Royal Purple: Social History and the Historians," *History Workshop* 7 (spring 1979): 66–94. For a complaint from the right, see Gertrude Himmelfarb, *The New History and the Old: Critical Essays and Reappraisals* (Cambridge, Mass., 1987); and Himmelfarb, "Some Reflections on the New History," *American Historical Review* 94 (June 1989): 661–70. Rejoinders to Himmelfarb are offered by Lawrence Levine, "The

Unpredictable Past: Reflections on Recent American Historiography," and Joan Wallach Scott, "History in Crisis? The Others' Side of the Story," *ibid.*, 671–79, 680–92. For "webs of cultural discourse," see Fox and Lears, *Power of Culture*, 4.

43. See Gordon Craig, "Political History," *Daedalus* 100 (spring 1971): 323–38, for an early recognition of these trends. Manifestos supporting the virtues of social history long predated the 1960s. One of the most powerful came from Frederick Jackson Turner, "The Significance of History," *Wisconsin Journal of Education* 21 (October–November 1891): 230–34, 253–56. But the modern efflorescence of social history occurred after 1960.

44. Some labor historians have made a point of paying close attention to the state. See Nelson Lichtenstein, *Labor's War at Home: The CIO in World War II* (New York, 1982); Melvyn Dubofsky, *The State and Labor in Modern America* (Chapel Hill, N.C., 1994); and Robert Zieger, *The CIO, 1935–1955* (Chapel Hill, N.C., 1995).

45. For sociology see Andrew Abbott, "History and Sociology: The Lost Synthesis," *Social Science History* 15 (summer 1991), 201–38; and Dennis Wrong, "The Present Condition of American Sociology: A Review Article," *Comparative Studies in Society and History* 35 (January 1993): 183–96. For political science see Ira Katznelson, "The State to the Rescue? Political Science and History Reconnect," *Social Research* 59 (winter 1992): 719–37; and David Brian Robertson, "The Return to History and the New Institutionalism in American Political Science," *Social Science History* 17 (spring 1993): 1–36. Another relevant article, sounding a call for cooperation between historians and political scientists, is William Leuchtenburg, "The Pertinence of Political History: Reflections on the Significance of the State in America," *Journal of American History* 73 (December 1986): 585–600.

46. *Clio* (the newsletter of the section) 3 (fall & winter, 1992/1993): 10. The section also sponsored ten sessions at the September 1992 meeting of the American Political Science Association. By contrast, some historians of twentieth-century politics have felt marginalized at conventions of the Organization of American Historians. See William Leuchtenburg, "The Uses and Abuses of History," *History and Politics Newsletter* 2 (fall 1991): 6–8. This was the earlier title of *Clio*.

47. Eric Nordlinger, *On the Autonomy of the Democratic State* (Cambridge, Mass., 1981); Karen Orren and Stephen Skowronek, "Editors' Preface," *Studies in American Political Development* 1 (1986): 1; Skowronek, *Building a New American State: The Expansion of Administrative Capacities, 1877–1920* (New York, 1982); Theda Skocpol, "Bringing the State Back In: Strategies of Analysis in Current Research," in Peter Evans, Dietrich Rueschmeyer, and Skocpol, eds., *Bringing the State Back In* (New York, 1975), 9ff.

48. A collection of essays in this field—most by historians—is Donald Critchlow and Ellis Hawley, eds., *Federal Social Policy: The Historical Dimension* (University Park, Pa., 1988). Key monographs include Hugh Davis Graham, *The Civil Rights Era: Origins and Development of National Policy, 1960–1972* (New York, 1990); and Theda Skocpol, *Protecting Soldiers and Mothers: The Political Origins of Social Policy in the United States* (Cambridge, Mass., 1992) (on old age and mothers' pensions from the 1870s into the 1920s).

49. See Samuel Hays, "Society and Politics: Politics and Society," *Journal of Interdisciplinary History* 15 (winter 1985), 481–99; and J. Morgan Kousser, "Restoring Politics to Political History," *ibid.*, 12 (spring 1982), 569–95.

50. Abbott, "History and Sociology," 234.

51. In addition to the works by Garrow, Branch, and Lemann cited above, important historical studies of race relations and civil rights include John Dittmer, *Local People: The Struggle for Civil Rights in Mississippi* (Urbana, Ill., 1994); William Chafe, *Civilities and Civil Rights: Greensboro, North Carolina and the Black Struggle for Freedom* (New York, 1980); Steven Lawson, *Running for Freedom: Civil Rights and Black Politics in America since 1941*

(Philadelphia, 1991); Clayborne Carson, *In Struggle: SNCC and the Black Awakening of the 1960s* (Cambridge, Mass., 1981); Harvard Sitkoff, *The Struggle for Black Equality, 1954–1992* (New York, 1992); and Robert Weisbrot, *Freedom Bound: A History of America's Civil Rights Movement* (New York, 1990). Some of the many works on the left include James Miller, *"Democracy Is in the Streets": From Port Huron to the Siege of Chicago* (New York, 1987); Maurice Isserman, *If I Had a Hammer: The Death of the Old Left and the Birth of the New Left* (New York, 1987); and John Diggins, *The Rise and Fall of the American Left* (New York, 1992). On women and feminism, see William Chafe, *The Paradox of Change: American Women in the Twentieth Century* (New York, 1991); Sara Evans, *Personal Politics: The Roots of Women's Liberation in the Civil Rights Movement and the New Left* (New York, 1979); Nancy Cott, *The Grounding of Modern Feminism* (New Haven, 1987); and Ann Shola Orloff, "Gender in Early U.S. Social Policy," *Journal of Policy History* 3 (1991): 249–80.

52. For such a lament see Hugh Davis Graham, "The Stunted Career of Public History, A Critique and an Agenda," *The Public Historian* 15 (spring 1993): 15–37. Also "Roundtable: Responses to Hugh Davis Graham's 'The Stunted Career of Policy History: A Critique and an Agenda,'" *ibid.*, 15 (fall 1993): 51–81.

53. Diplomatic historians, however, have responded to their marginalization within the profession by establishing strong institutions of their own, notably the Society for Historians of Foreign Relations, which had a membership of more than fifteen hundred in 1992, and sponsors an excellent journal, *Diplomatic History* (1977–). They have also forged links with students of international relations, most of them in the discipline of political science. See Michael Hunt, "The Long Crisis in U.S. Diplomatic History: Coming to Closure," *Diplomatic History* 16 (winter 1992): 115–40; "Writing the History of U.S. Foreign Relations: A Symposium," *ibid.*, 14 (fall 1990): 553–605; and Jerald Combs, *American Diplomatic History: Two Centuries of Changing Interpretations* (Berkeley, 1983).

54. Thomas Bender, "'Venturesome and Cautious': American History in the 1990s," *American Historical Review* 81 (December 1994): 992–1003; Bender, "Wholes and Parts: The Need for Synthesis in American History," *Journal of American History* 73 (June 1986): 120–36.

55. Vaudagna, "American History at Home and Abroad," 1165–67, suggests that American historians (taking for granted their freedom and affluence) are more critical of their own recent history than are non-Americans working in the field.

56. A point made by Carl Degler, "In Pursuit of an American History," *American Historical Review* 92 (February 1987): 1–12; and Raymond Grew, "The Comparative Weakness of American History," *Journal of Interdisciplinary History* 16 (summer 1985): 87–101. See also George Fredrickson, "What Is the New History?" *Dissent* (summer 1991): 428–32.

57. Again there are exceptions to this generalization. Well-regarded comparative works include Fredrickson, *White Supremacy: A Comparative Study in American and South African History* (New York, 1981); Fredrickson, *Black Liberation: A Comparative History of Black Ideology* (New York, 1995); James Kloppenberg, *Uncertain Victory: Social Democracy and Progressivism in Europe and America, 1870–1920* (New York, 1986); Alfred Chandler, Jr., *Scale and Scope: The Dynamics of Industrial Capitalism* (Cambridge, Mass., 1990); and Susan Fainstein et al., eds., *Divided Cities: New York and London in the Contemporary World* (Cambridge, Mass., 1992).

58. For recent scholarship on developments in technology and industrial organization, see Alfred Chandler, Jr., "The Competitive Performance of U.S. Industrial Enterprises since the Second World War," *Business History Review* 68 (spring 1994): 1–72. For studies of business-political relations see Otis Graham, *Losing Time: The Industrial Policy Debate* (Cambridge, Mass., 1992); Kim McQuaid, *Uneasy Partners: Big Business in American Politics*,

1945–1990 (Baltimore, 1994); Elizabeth Fones-Wolf, *Selling Free Enterprise: The Business Assault on Labor and Liberalism, 1945–1960* (Urbana, 1994); and David Vogel, *Fluctuating Fortunes: The Political Power of Business in America* (New York, 1989). For works concerning science and industry, see David Noble, *Forces of Production: A Social History of Industrial Automation* (New York, 1984); Noble, *America by Design: Science, Technology, and the Rise of Corporate Capitalism* (New York, 1979); David Nye, *Electrifying America: Social Meanings of a New Technology, 1880–1940* (Cambridge, Mass., 1990); Stuart Leslie, *The Cold War and American Science: The Military-Industrial Complex at MIT and Stanford* (New York, 1993); and James Cortada, *Before the Computer: IBM, NCR, Burroughs, and Remington Rand, and the Industry They Created, 1886–1956* (Princeton, 1993). For developments in medicine and society, see Allan Brandt, *No Magic Bullet: A Social History of Venereal Disease in the United States since 1880* (New York, 1985); James Jones, *Bad Blood: The Tuskegee Syphilis Experiment* (New York, 1993); James Patterson, *The Dread Disease: Cancer and Modern American Culture* (Cambridge, Mass., 1987); Joan Brumberg, *Fasting Girls: The Emergence of Anorexia Nervosa as a Modern Disease* (Cambridge, Mass., 1988); David Rothman, *Strangers at the Bedside: A History of How Law and Ethics Transformed Medical Decision Making* (New York, 1991); and Robert Proctor, *Cancer Wars: How Politics Shapes What We Know and Don't Know about Cancer* (New York, 1995).

59. For example, Lynn Gordon, *Gender and Higher Education in the Progressive Era* (New Haven, 1990); Paula Fass, *Outside In: Minorities and the Transformation of American Education* (New York, 1989); Arthur Zilversmit, *Changing Schools: Progressive Educational Theory and Practice, 1930–1960* (Chicago, 1993); Roger Geiger, *To Advance Knowledge: The Growth of the American Research University* (New York, 1986); Geiger, *Research and Relevant Knowledge: American Research Universities since World War II* (New York, 1993); and Diane Ravitch, *The Troubled Crusade: American Education, 1945–1980* (New York, 1983).

60. See Paul Boyer, *When Time Shall Be No More: Prophecy Belief in Modern American Culture* (New York, 1992); George Marsden, *Fundamentalism and American Culture: The Shaping of Twentieth-Century Evangelicalism, 1870–1925* (New York, 1980); Leo Ribuffo, *The Old Christian Right: The Protestant Far Right from the Great Depression to the Cold War* (Philadelphia, 1983); Michael Lacey, ed., *Religion and Twentieth Century American Intellectual Life* (Washington, D.C., 1989); Ronald Numbers, *The Creationists: The Evolution of Scientific Creationism* (New York, 1992); Robert Wuthnow, *The Restructuring of American Religion: Society and Faith since World War II* (Princeton, 1988); and Philip Gleason, *Keeping the Faith: American Catholicism Past and Present* (South Bend, 1987). Major books in twentieth-century intellectual history include Richard Wightman Fox, *Reinhold Niebuhr: A Biography* (New York, 1986); Howard Brick, *Thorstein Veblen and His Critics, 1891–1963* (Princeton, 1992); Robert Westbrook, *John Dewey and American Democracy* (Ithaca, 1991); Dorothy Ross, *The Origins of Social Science* (New York, 1991); Michael Wreszin, *A Rebel in Defense of Tradition: The Life and Politics of Dwight Macdonald* (New York, 1994); and David Lewis, *W. E. B. DuBois: Biography of a Race, 1868–1919* (New York, 1993).

61. See William Cronon et al., eds., *Under an Open Sky: Rethinking America's Western Past* (New York, 1992); Samuel Hays, *Beauty, Health, and Permanence: Environmental Politics in the United States, 1955–1985* (New York, 1987); Hays, "The New Environmental West," *Journal of Policy History* 3 (1991): 223–48; Michael Lacey, ed., *Government and Environmental Politics: Essays on Historical Developments since World War Two* (Washington, D.C., 1989); Robert Dorman, *Revolt of the Provinces: The Regionalist Movement in America, 1920–1945* (Chapel Hill, N.C., 1993); Jon Teaford, *Cities of the Heartland: The Rise and Fall of the Industrial Midwest* (Bloomington, Ind., 1993); Carl Abbott, *The Metropolitan Frontier: Cities in the Modern American West* (Tucson, 1993); John Findlay, *Magic Lands: Western*

Cityscapes and American Culture after 1940 (Berkeley, 1990); Gerald Nash, *World War II and the West: Reshaping the Economy* (Lincoln, Neb., 1990); Donald Worster, *Rivers of Empire: Water, Aridity, and the Growth of the American West* (New York, 1986); and Bruce Schulman, *From Cotton Belt to Sunbelt: Federal Policy, Economic Development, and the Transformation of the South, 1938–1980* (New York, 1991).

62. In addition to works noted earlier—by Hall, Lichtenstein, Montgomery, Dubofsky, Gerstle, and others—see Steve Fraser, *Labor Will Rule: Sidney Hillman and the Rise of American Labor* (New York, 1991); Eileen Boris, *Home to Work: Motherhood and the Politics of Industrial Homework in the United States* (New York, 1994); and Richard Oestreicher, “Urban Working-Class Political Behavior and Theories of American Electoral Politics, 1870–1940,” *Journal of American History* 74 (March 1988): 1257–86.

63. For example, Richard Kluger, *Simple Justice: The History of Brown v. Board of Education and Black America's Struggle for Equality* (New York, 1976); Mark Tushnet, *Making Civil Rights Law: Thurgood Marshall and the Supreme Court, 1936–1961* (New York, 1994); Laura Kalman, *Abe Fortas: A Biography* (New Haven, 1990); Stanley Kutler, *The American Inquisition: Justice and Injustice in the Cold War* (New York, 1982); Melvin Urofsky, *A Conflict of Rights: The Supreme Court and Affirmative Action* (New York, 1991); Richard Polenberg, *Fighting Faiths: The Abrams Case, the Supreme Court, and Free Speech* (New York, 1987); and William Leuchtenburg, *The Supreme Court Reborn: The Constitutional Revolution in the Age of Roosevelt* (New York, 1995). Kalman, Kutler, Urofsky, Polenberg, and Leuchtenburg are trained as historians.

64. Lizabeth Cohen, *Making a New Deal: Industrial Workers in Chicago, 1919–1939* (New York, 1990). See also Dana Frank, *Purchasing Power, Consumer Organizing, Gender, and the Seattle Labor Movement, 1919–1929* (New York, 1994).

65. Steven Biel, *Down with the Old Canoe: A Cultural History of the Titanic Disaster* (New York, 1996).

66. Brandt, *No Magic Bullet*. See also Proctor, *Cancer Wars*.

67. Nightingale, *On the Edge: A History of Poor Black Children and Their American Dreams* (New York, 1993).

68. James Goodman, *Stories of Scottsboro* (New York, 1994).

69. Paula Baker, *The Moral Frameworks of Public Life: Gender, Politics, and the State in Rural New York, 1870–1930* (New York, 1991); Linda Gordon, *Pitied but Not Entitled: Single Mothers and the History of Welfare, 1890–1935* (New York, 1994). See also Gordon, ed., *Women, the State, and Welfare* (Madison, Wis., 1990); Ellen Fitzpatrick, *Women Social Scientists and Progressive Reform* (New York, 1990); Robyn Muncy, *Creating a Female Dominion in American Reform, 1890–1935* (New York, 1991); Vivien Hart, *Bound by Our Constitution: Women, Workers, and the Minimum Wage* (Princeton, 1994); and Evans, Rueschemeyer, and Skocpol, eds., *Bringing the State Back In*. For efforts to build bridges between women's history and diplomatic history, see “Culture, Gender, and Foreign Policy: A Symposium,” *Diplomatic History* 18 (winter 1994): 47–124, especially Emily Rosenberg, “‘Foreign Affairs’ after World War II: Connecting Sexual and International Politics,” 59–70.

70. Hollinger, “How Wide the Circle of the ‘We’?”

Western Civilization

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"THE RISE OF 'Western Civ' is one of the great success stories in the history of the historical profession in America," declares Gilbert Allardyce in a seminal essay designed not to praise "this wilted course" but to bury it. Yet the reasons Allardyce advances to explain why "the world has outgrown the old Western Civ idea"¹ could easily be turned round to explain the enduring use of it.

Like most chroniclers of Western Civ, Allardyce relates the birth of the course to the fin-de-siècle struggle between a "chaotic" elective system and those reacting against atomizing specialization and professionalism. A general education curriculum, some argued, should combat fragmentation and provide a common learning experience—above all exposure to that Western tradition that could equip citizens with the sense of a common identity, common past, and common purpose. American history had been about leaving the Old World behind. As the nineteenth century ended, as the twentieth century began, Americans started to rediscover the past that they had shed, no longer as restrictive but increasingly as inspiring or, at least, suggestive. More Americans traveled, more Americans traded abroad; more economic, social, political involvement in Europe meant more attention devoted to Europe. American culture was the offshoot of European culture. Growing interest in European culture brought growing interest in those origins that were the origins of American culture too, and in the sociopolitical evolution that was held to account for both.

There were good reasons for some kind of synthetic exposure that would provide young Americans with a sense of their past as part of a common Western experience, a common civilization that reflected and affirmed the forward march of liberty, democracy, and progress. But there was more to it.

Under the elective system, a professor of literature at Columbia declared, "an incomplete knowledge of enough courses leads to a degree."² But the same was true of the public and private schools whose graduates reached college. As Charles Homer Haskins reported to the American Historical Association in 1906, "the reason why we introduced this general education course on European history . . . is because students did not bring it to college with them."³ Ninety years ago, just as today, at Haskins's Harvard as in most other schools, general education courses were remedial. They remained so. By the time of the First World War, "even those soldiers who are neither illiterate nor unable to command the English language showed to a distressing degree the inefficiency of our popular education."⁴ All evidence testifies to an enduring problem. In recent years *New*

York Times surveys of college freshmen continue to reveal a dismal knowledge of history;⁵ and some college teachers of history react by proposing history courses dedicated to the development of missing skills, like “a European survey course which stresses writing and reasoning skills.”⁶ And a bit of history too, no doubt.

This is the background against which Western Civ and proto-Western Civ courses were introduced. Part of a program of general education designed to counter the alleged shortcomings of the elective system, Western Civ and its predecessors were also part of a broader campaign to socialize, “civilize,” and integrate immigrants and members of social groups entering college with little or no exposure to the general—dominant—culture. They were also a kind of civics: an education for democracy and for a more responsible awareness of the wider world, its background and its problems. They represented the affirmation of a national culture, rooted in the Mediterranean and the European past. This is what makes a college course like Western Civ so peculiarly American. Europeans prefer to teach and write national history. Americans do that too; but they conceive and present the national past as prolonged backward not into the British past alone but into a broader Western tradition, originating in the Fertile Crescent and in the Mediterranean of Greece and Rome, where the groundwork was laid for references and memories that resurface in the conflicts and creations of the Middle Ages, Renaissance, Reformation, Enlightenment, and so on.

French, British, Italian, German historians recognize similar sources. But French, British, Italian, German historians treat Europe and the wider world as context, whereas Americans treat Europe, and especially Western Europe, as text: as “roots.” Western Civ provides the inescapable background of national history, to be followed in the past two hundred years by the international context within which our own national history evolves. Just how American this concept became can be seen from the fact that, whereas Winston Churchill never referred to Western Civilization in his great war and postwar speeches, preferring terms like “Western Democracies” or “Christian Civilization,” Dean Acheson, in similar public circumstances, spoke of “that Western Civilization which we share with other nations bordering the North Atlantic.”⁷

The shape that Western Civ would take was laid down in 1903, when Harvard’s Outline of General History course was restructured to two hours of lecture and two section-hours taught by graduate assistants offering quizzes, map drills, perhaps a modicum of discussion. In 1904, Charles Homer Haskins (a medievalist) took over the course and introduced the use of original source material, and Western Civ never looked back. But it would be the First World War that turned General History into a survey of civilization past and present, designed to expose all young men to the history and culture of the warring nations and then, the war at an end, to turn students into “citizens who shall be safe for democracy.”⁸

The War Issues or War Aims courses that some 540 colleges taught in 1917 and 1918 continued in 1919 and after as Peace Aims courses, offered by 300 or more of the original 540. This initial lecture-and-discussion prototype soon received a more academic label, like the Introduction to Contemporary Civilization in the West, or CC, that Columbia began to require of all freshmen in 1919.

CC was “concerned with the appearance and development of certain institutions and of certain problems which are the common possession of the peoples of Europe and the United States, and to a greater and greater degree of all the peoples of the world.”⁹ As the first course in social sciences to which Columbia students were exposed, it presented historical doings “not as isolated subject-matter but in their interrelations.” And it emphasized, as the course syllabus declared, “the cross-influence of economics and politics, the interpretative and directive functions of philosophy, and the integration of art and religion with the social fabric, and the merging of all these in the historical totality of each period. . . .”¹⁰

While large survey courses at Harvard tended to feature “great men teaching about great men,” the Columbia tradition broadcast a livelier and more socially conscious brand of history whose influence could be found as far as Stanford, where a course entitled Problems of Citizenship, instituted in 1920, was soon oriented toward history by Edward Eugene Robinson. In the early thirties Cit, as students called it, turned into a History of Western Civilization, dominated by a Columbia man, Max Savelle, and using the textbook of another Columbia man: Harry Elmer Barnes’s *History of Western Civilization*.

We do well to remember that war only accelerated or facilitated trends already visible before its outbreak. John Erskine, professor of literature at Columbia, remembered how “in the prewar years . . . we all became aware of increasing tension in the world and responded to it with a general speeding up of our serious activities. We hatched so many schemes for improving education and saving mankind that we bewildered ourselves.”¹¹ Erskine himself hatched one scheme with a great future. It turned about “the presentation of great authors and their works to young people,” who “knew little or nothing . . . about the Bible, or Homer, or Vergil, or Dante, or the other giants . . .”; and it was about the best-sellers of ancient times: great books “that ha[ve] meaning and continue to ha[ve] meaning, for a variety of people over a long period of time.”¹²

Unsuccessful at first, in 1920 Erskine at last persuaded the college faculty at Columbia to let him try out his Great Books course, in a class divided into discussion sections numbering between fifteen and thirty students, led by the like of Mortimer Adler, Mark Van Doren, Rexford Tugwell, Moses Hadas, and Clifton Fadiman. Continued at Columbia by teachers like Jacques Barzun and Lionel Trilling, carried to Chicago and elsewhere by Erskine’s instructors and students, Great Books (or Great Books by any other name!) reflected the same spirit that gave rise to Western Civ. Jacques Barzun, who took CC in 1923 and later taught it for ten years, also took Honors (Erskine’s Great Books course) between 1925 and 1927, before teaching it in turn. As he remembers in a letter of February 18, 1994:

Mortimer Adler was one of the instructors of a section of “Honors” and when he joined Hutchins at Chicago he carried the idea with him. . . . From there, the pattern spread to the Loop—evening groups of Chicago business men reading *Antigone* and arguing about law and justice; and further still, to Aspen after its establishment as a

cultural center, where men and women of all professions spent two weeks in Executive Seminars based on the same scheme and the same type of readings. At one of these I had the pleasure of guiding the minds of Justice Hugo Black at one side of the table and, on the other, of Walter Reuther of the Auto Workers Union, through the mazes of Hobbes and Montesquieu and the *Federalist*.

Would that CC or Western Civ could claim as much! Still, the record is honorable. Printed syllabi for courses offered at Chicago and Columbia between the wars reflect real sophistication. At Chicago, where Ferdinand Schevill began in 1909 to teach a course called History of Civilization (meaning Europe), we learn that his lectures of 1920 sought to show the progressive development of culture and to provide "a synthesis of human activities and knowledge, to counter the development of specialized disciplines." In 1927, after Schevill retired, the General Humanities course took the name History of Civilization. As the "Syllabus" of the new course, recast and broadened by Arthur P. Scott, declared: "the course in the Humanities deals principally with the intellectual, emotional, and artistic values in life. It limits itself in the main to Western Civilization and its Mediterranean antecedents, without denying the interest and importance of other civilizations, notably those of Asia."¹³ There as elsewhere, critical awareness of nationalism, imperialism, slavery, labor and industrial problems reflected the sensibilities of the interwar years—as indeed did the absence of women and minorities as specific topics of discussion. One has to avoid anachronism when viewing the work of other generations. But I remain impressed by their scope and by their perspective—as when imperialism, for example, described as conquest, rule, and exploitation of unwilling peoples, is identified not only in modern times but in the Ancient East "and more or less recurrent since."¹⁴ It would seem that American historians were "social," "synthetic," "critical," and self-critical much sooner than we give them credit for.

Courses generate texts; texts mirror courses more often than they inspire them. The best way to follow the evolution of Western Civ is by looking at the textbooks it inspired, most (though not all) of which bear out Ralph Waldo Emerson's dictum that each age must write its own books. And here too, as in syllabi, my first impression is that, far from dumbing down an ill-prepared public, America's textbook writers reflected a lively intent to share very current notions, interests, and attitudes, more at the cutting edge of the profession than at its duller end. This was American too.

In 1893 already, the same year in which he published his essay on the Frontier, Frederick Jackson Turner insisted that "each age writes the history of the past anew, with reference to the conditions uppermost in its own time." Turner's age of economic successes and preoccupations looked to economics, where the age of Guizot and Ranke had looked to politics. But Americans knew that there was more to history than economics. In the country of the common man, all human and social activities attracted more and earlier attention than elsewhere. Teaching at the university of Michigan in the late 1850s and the early sixties, Andrew Dickson White (later the first president of Cornell) was stimulated by an

atmosphere “in which history became less a matter of annals and more a record of the unfolding of humanity.”¹⁵ By the turn of the century, White was enlisting anthropology and comparative religion in the study of history, whilst at Columbia James Harvey Robinson insisted that all activities of man and all the “social” sciences that observed them were crucial adjuncts to a discipline more interested in conditions and institutions than in events.¹⁶ That discipline was *The New History*, title of a collection of essays that Robinson published in 1912 but that he and others had practiced, as M. Jourdain practiced prose, for a good while before that.

New Historians echoed the *historikerstreit* unfolding in fin-de-siècle Germany around the work of Karl Lamprecht, with its insistence on what would later be called mentalités: plumbing collective consciousness; synthesizing cultural enterprises at a time when music and art, like politics and the economy, were treated as separate entities; trying to interpret how we got here from there (*wie ist es eigentlich geworden?*). Significantly, Lamprecht’s most comprehensive statement of his belief in history as “primarily a socio-psychological science” would be delivered in the United States, where he was invited to speak at the St. Louis World’s Fair of 1904.¹⁷

If German pronouncements loomed large, that may have been because they reinforced native tendencies. One always looks to others for ideas but, even more, for confirmation of one’s own ideas. Leonard Krieger has referred to the “veritable flood of textbooks” appearing before 1914.¹⁸ James Harvey Robinson may be regarded as a major contributor to this flow. Robinson had been trained at Harvard and in Germany, but neither seems to have marked him deeply. He taught at the university of Pennsylvania in the early nineties, then went to Barnard and Columbia, which he left only in 1919 to help found the New School for Social Research. Robinson’s intellectual history course at Columbia was so famous that only the largest amphitheatre on campus could accommodate it. While not participating directly in the creation of Contemporary Civilization in 1919, he was regarded as one of the founders of a course with which many of his students were involved. He certainly personifies the New History, of which Krieger regards textbooks as a characteristic expression “appropriate to their philosophy of history as an instrument of social reform,”¹⁹ and, one may add, propaganda. Robinson’s contributions broadened their scope and effectiveness.

Late-nineteenth-century textbooks had been vehicles for learning by rote, in classes where lectures often repeated reading assignments and recitations repeated both. Furthermore texts, even sophisticated texts, were about politics. As Charles Seignobos, a learned and worldly man, insisted in the preface to his *Political History of Europe Since 1814* (New York, 1899), “I have avoided all social phenomena that have had no direct effect on political life: art, science, literature, religion, private manners, and customs.” This self-denying sobriety was the very contrary of Robinson’s view, in which history could no longer be past politics, or a string of past events: “Our so-called standard works on history deal at length with kings and popes, courtiers and statesmen, wars waged for territory or thrones, laws passed by princes and parliaments. But these matters form only a

very small part of history. . . ." Old stuff now, but exciting in its day, as is Robinson's conclusion: "It is clear that our interests are changing and consequently the kind of questions we ask the past to answer [are changing too]."²⁰

Old questions, old answers no longer suited new times that called for a history fit for the Common Man. That history, moreover, should no longer cling to inflexible centuries, to invented epochs like the Middle Ages or the Renaissance which Haskins had shown to be very dubious notions, even to generational categories, all of which were subject to time lags, overlappings, and significant variations in space. So, where official history still followed Thucydides, Robinson beat a path toward Herodotus. Conditions as well as events, transitional periods as well as crises, developments in the past, facets of the present, all these and more were grist to history's mill. All history was the business of history. To plumb such vast territories, the historian must use "the newer sciences of man": anthropology, archaeology historical and prehistorical, social and animal psychology, sociology, political economy, the comparative study of religions, but also the natural sciences which are not our rivals but our allies.²¹ And, as a first step toward comprehensive history, Robinson, while still at the University of Pennsylvania, initiated a series of Translations and Reprints from the Original Sources of European History (University of Pennsylvania Press, 1894–99).

Then the textbooks started. In 1902, an *Introduction to the History of Western Europe* would be seconded by two volumes of *Readings in European History* (1904). From 1902 to 1924, the *Introduction* sold between 10,000 and 30,000 copies a year; the *Readings* sold about 83,000 copies in all: not a bad score. In 1907, together with his student, friend, and colleague Charles Beard, Robinson published *The Development of Modern Europe: An Introduction to the Study of Current History*, which, as the title indicates, subordinated past to present. The revised text of 1930 is especially attractive in the way it uses works of fiction as historical sources, and no less for the serious attention it shows to women: Mrs. Radcliffe, Jane Austen, George Eliot, the Brontë sisters of course; but also woman suffrage (six index entries), women's employment (two entries), women's rights (one entry). Not bad for its day.

A partial list of Robinson's students reads like an honor roll of interwar American historians of Europe, and of textbook writers too: Harry Elmer Barnes, Charles Beard, Carl Becker, Carlton Hayes, John H. Randall, Salwyn Schapiro, James Shotwell, Preserved Smith, Lynn Thorndyke; historians of the world like the brothers G. W. and J. B. Botsford; of civilization like Shotwell and Thorndyke; of the making of the modern mind like Randall and Preserved Smith; but above all narrators of European history, justified as "the seat of that continuous high civilization which we call 'western'—which has come to be the distinctive civilization of the American continents as well as of Europe."²²

In the hands of good students the scope of history alters and expands. "Old landmarks drop out of sight," James Shotwell explained in the fourteenth edition of the *Encyclopedia Britannica*, "perspective changes. . . ." Hayes and Schapiro devoted more attention to the industrial revolution and to the problems it created. Written during the Great War and revised in 1926, Hayes's *Political and Social*

History of Modern Europe was followed in 1938 by a *Political and Cultural History of Modern Europe*. The shift from “social” to the more comprehensive “cultural” was significant. But more changed than nuances. The 1926 version offered maps but no illustrations; published as the thirties ended, its successor reveled in illustrations, many of them reproductions of Gilray, an eighteenth-century draftsman whose prejudices came close to those of the author. Perspective opened up. Robinson’s *Ordeal of Civilization* began with the breakup of the Roman Empire and the rise of Christianity. Harry Elmer Barnes’s *History of Western Civilization* (1935) begins with primitive societies, notes the agricultural revolution of the Neolithic Age, goes well beyond outdated versions of a heritage beginning only with the Greeks.

The 1930 edition of Columbia’s *Introduction to Contemporary Civilization in the West* describes the postwar United States as “a scene of complacent well-being that verges upon smug self-satisfaction.”²³ We know that this was changing even as the comment went to print. No one reflected this more vividly than Barnes, a reforming penologist, pacifist, and socialist. His *History of Western Civilization* and his *Intellectual and Cultural History of the Western World* (1937; revised 1941) make clear the author’s belief that the mass slaughter of modern war challenged the doctrine of progress; that capitalism, nationalism, democracy, and religion were on their last legs; that militarism, crime, poverty, and “sexual misery” threatened an end to civilization.²⁴

Robinson’s apostolic succession appears at its most striking in the work of Carl Becker and of Becker’s students: Geoffrey Bruun, Leo Gershoy, Louis Gottschalk and Robert Palmer, a doughty quartet of editors and writers. Becker wrote relatively little, and the texts that he wrote (though they made him independently comfortable)²⁵ were meant for high school use. His *Modern History* (1931) was subtitled *The Rise of a Democratic, Scientific and Industrialized Civilization*. Like *The Story of Civilization* (1940), it reflected Becker’s secularism, pragmatism, relativism, gentle humor, and brilliant writing. Where Barnes’s civilization lurches dangerously toward self-dug ditches, Becker’s continues to advance thanks to the growth of scientific knowledge, the development of humane feelings, the persistence of democratic ideas, the spread of economic interdependence, the marriage of nationalism and liberalism. One would think that Cornell, where Becker taught, was beyond the reach of press or radio. It was not, however, beyond the reach of prejudice. This gentlest of men was denounced as a Communist in 1935 and again in 1940; but his publishers, Silver, Burdett & Co., successfully fought off both attacks. Their author’s religious skepticism, on the other hand, created more serious problems. Becker had long believed that less religion and more manners would improve a lot of “good” people. When his treatment of indulgences offended Roman Catholics on school boards, notably in New York City, the offending passages had to be reworded.²⁶

How subject were authors and publishers to ideological and political pressures? We can hazard a sketchy answer based on the correspondence between a successful author, Edward McNall Burns, and his editors at W. W. Norton.²⁷ The first edition of *Western Civilizations* was copyrighted in 1941, and, by the time of

the second edition (1945), some forty colleges and universities (including Stanford, MIT, Vassar, Pomona, Emory, and Hunter) had adopted the book. But the author's views stirred criticism. As the publisher wrote in January 1942, "we must face the fact that your book will be hurt by your treatment of nationalism, war, and [by] the last hundred pages. Allen & Unwin were very keen on an English edition until they came to this last section." Despite other similar criticism of his Quaker pacifism, Burns held fast, and his publisher's files of the 1950s reveal frequent objections to the book's "propagandizing for pacifism." A letter of September 1956 quotes a Stanford critic: "Burns sticks his neck out on his little hobby horses: anti-religious feeling and pacifism." It was from the former that the real trouble came.

In November 1946, a priest in Newark, New Jersey, Father Toohey, while denouncing state education as atheistic, cited Burns's *Western Civilizations*, which "denies the divinity of Christ." The Convention of Holy Name Societies of Northern New Jersey adopted resolutions that denounced the United Nations and also, for good measure, Burns's text as "containing material offensive to all religious faiths." Responding to their pressure, the Colleges of New Jersey prohibited class use of the book after February 1947. Since Burns taught at Rutgers, this meant that he could not use his own book, any more than his students and colleagues could use it in other New Jersey establishments. Faced with a serious loss of sales, Norton's editors wrote to the New Jersey Committee of Education promising revisions "to meet in part at least" objections raised by "certain elements of the Roman Catholic Church" to remarks bearing on the divinity of Christ and the chastity of the clergy. Concurrently and more constructively, a Fordham historian was hired to go over the third edition, which he found "pervaded by atheistic naturalism and a crude Darwinism." The crucial suggestions of Professor Ross Hoffman's report must have borne fruit, since in 1950 New Jersey reauthorized use of Burns's text. Burns remained religiously suspect ("he must be a Jew"), but acceptable enough for more successful editions to continue to appear, as well as a companion text, *World Civilizations*, written with Philip L. Ralph.

We do well to remember that attempts to impose some kind of conformity are not new in the land, any more than attempts to challenge conformity. In 1835, Tocqueville could think of no other country where one found less independence of thought and freedom of discussion. That was American provincialism, and the restrictive side of its democracy. Provincialism waned, democracy grew more permissive, and censorship and self-censorship persisted; only terms and notions changed: in the days when one could be rude about Jews and blacks, one had to be careful what one said about Communists and Catholics. Some moral minority or majority has always been there, some intellectual and cultural hierarchies have always been menaced and asserted, some militants have always militated for their version of what is true and just, and always sparked opposition as militant and shrill. Yet authors and publishers of textbooks that are *also* commercial enterprises are likely to heed invitations and demands relevant to adoptions and sales. Euphemization of language, edulcoration of ideas, existed long before the wars of multiculturalism and of political correctness.

Burns's problems with critics of his religious views confirm my own impressions while teaching Western Civ from the McCarthy era of the early 1950s to the 1980s: religion proved a more sensitive subject than politics properly speaking. Becker's political liberalism had proved less troublesome than his secularist skepticism. Even so, and whatever the stumbling blocks, most textbooks I have seen, beginning with those of Robinson, reflect strongly held views. There is nothing bland about them, and they address readers as intelligent adults open to information and enlightenment. The author's personal orientation, whether Marxist with Barnes, pacifist with Burns, Catholic with Carlton Hayes, strikes me less as intrusive than as adding flavor and interest to urbane discourse.

This was going to change somewhat when more undergraduates were required to attend introductory surveys where they were told "to read on successive Mondays, Wednesdays, and Fridays, successive chapters in a prescribed textbook" destined to remain "the principal source of information for most students." That is what Carl Becker describes with resignation and some distaste in his editor's introduction to just such a book: Wallace Ferguson and Geoffrey Bruun's *Survey of European Civilization* (1936, 1939, 1942). The *Survey* was a true textbook as we have come to expect them—impersonal, detached, economical, not very memorable, but profitably enduring for Houghton Mifflin. After Becker's death, "editorship" would pass to William Langer and then to Leo Gershoy, a Becker student like Bruun. But, while the tone might change and differ from author to author, while coverage might begin with the Renaissance or Rome or Egypt or prehistoric times (many texts worked their way backward from one edition to another), one assumption remained constant.

As Ferguson and Bruun put it, Europe represented "the logical center and European history the logical axis for what is now almost a world civilization."²⁸ By their time, first the approach, then the experience of a second world war had made Western Civ steadily more relevant (universities like Iowa and Ohio introduced it in 1943, UCLA in 1950),²⁹ and the European origins of American history especially significant. Some of the great teachers of Western Civ were themselves exiled Europeans, like Ferdinand Schevill and Karl Weinberg in Chicago. It was one of these, George Mosse, who explained to the American Historical Association in 1948 that courses in modern European or United States history were no longer enough. Freshmen needed to understand the relation of Europe and America in order to appreciate America's share in the general Europeanization of the world: "the student thus, from the very start, obtains a new sense of proportion as far as the American development is concerned."³⁰

So Western Civ remained steadfastly Eurocentric, but that was pretty much all that it *remained*. Cutting the cloth to fit the fashions that altered with the perceptions and problems of the moment, lectures and texts shed the confidence of the 1920s for the doubts and self-criticism of a less sanguine postwar. Change did not necessarily mean progress; progress did not illumine the history of social processes. As Burns wrote to his editor in March 1956, "we live in an age of chaos, not of order." What would he have written in 1986? Yet disorder made for choice or, at least, for variety. Great men, great ideas, great processes like urbanization,

or the succession and leapfrog of ideas, all found their chroniclers and readers. Allardyce gives the impression that, after World War II, Western Civ shifted from facts to the interpretation of facts, from claims of objectivity to proclamations of relativity, from narrative to analysis.³¹ But most of these novelties (or aspirations) could already be found between the wars, indeed in the New History that preceded the First World War. Some of the excellent suggestions for improving Western Civ advanced at a 1985 conference titled "What Americans Should Know" simply rehearsed approaches pioneered by Robinson: more primary documents, more comparative treatment, more contemporary references, more insistence that values are relative, more broad themes (how can Western Civ escape them?), and less parochialism—which was just what, each in his own way, Barnes, Becker, Carlton Hayes, and Mosse stood for.³²

You can't blame historians for not reading old textbooks; it's all they can do to keep up with new ones. But we do keep reinventing the inventions of the past, even though our brio is less than that of our predecessors. And we do add new apprehensions. The experience of growth had inspired a confidence based on growth. The experience of conflict inspired interpretations based on conflict. Looming doldrums reinforced self-doubt. More recently, challenges to Western hegemony call for more attention to the challengers; challenges to a once unreflective domination evoke once subordinate groups that must be brought to notice. Cultural unity and integration are pressed out, cultural differences and disintegration march in; at least in theory. When, in 1982, participants in the American Historical Association discussion opened by Allardyce called for less parochialism, that was not in order to expose students to some transatlantic sophistication but rather, as Carolyn Lougee put it, "to formulate a vision of coherent diversity and cultural pluralism."³³ Easier said than done.

The history of history is—also—the history of history's taking notice of hitherto invisible or irrelevant groups: the people, the workers, the masses, most of whom began to get their due before and after the First World War. After the 1960s, new claimants for notice battered at the gates. Typically, in 1979, Abby Kleinbaum raised the question of women's history and the Western Civilization survey: the traditional survey ignored the majority—not just women but slaves and other groups that did not fit the popular stereotype of progress toward more freedom and more goods.³⁴ In fact, by 1979 that stereotype was far from popular, and a good few surveys hardly ignored either women or slaves. The index of my own *Modern History of Europe*, written in the late 1960s, published in 1971, sports nine entries dealing with women, including a five-page section about their emancipation; and twenty-nine entries dealing with slaves and slavery, rather more than what socialism gets. More representative, perhaps, the 1976 edition of Walbank, Taylor, and Bailkey's *Civilization Past and Present* includes a three-page "profile" on women in history and the wrongs they suffered. Its epilogue reflects a fraught, self-questioning mood, with two and one-half pages devoted to 1984, three on the ecological crunch, two and one-half on the anguish of change (alienation, crime), and the remainder dwelling mostly on threats to the human condition.

The fact is that one can always find what one is looking for; and Abby Kleinbaum was more right than wrong. A cursory survey reveals little or no attention paid to women in representative texts and readings of the 1950s and sixties, with one suggestive exception: Leon Bernard and Theodore Hodges's *Readings in European History* (Notre Dame, 1958). Edited by two Catholic historians, it includes a Christian martyr (Blandina), Joan of Arc, Francesca da Rimini chatting to Dante, a witch, and several crowned heads: Catherine of Aragon, Catherine the Great, Maria-Theresa. Saints, sinners, and great ladies: the Catholic church knows where its bread is buttered.

But things were going to change. Between 1956 and 1978, women's index entries in Robert Palmer and Joel Colton's classic text increased from nine to sixteen. Between 1959 and 1990, selections from women in my own *Western Tradition* rose from five to seventeen. Other things changed too. In 1929 as in 1907, Robinson and Beard's *Development of Modern Europe* contained maps but no illustrations. In 1942, Ferguson and Bruun's *Survey of European Civilization* offered black-and-white illustrations as well as maps. By 1957, Brinton, Christopher, and Wolf's *Modern Civilization* boasted illustrations almost as sophisticated as the text. Instructor's manuals were introduced to cater to the needs or to the sloth of those who decide what texts to adopt or who, the text once adopted, welcome help in teaching it.

Design increasingly complements substance, enhances it, sometimes replaces it. Swiftly revised editions, intended to counter the used-book market, accumulate creative gimmicks. Production values take over: art essays accompanying splendid color plates; complex cartographic essays like that by Michael Conzen in Chambers et al., *The Western Experience*; or "part-opening time-lines, a key topic list at the beginning of each chapter, thought-provoking questions with each of the more than 200 primary-source selections in the text, and chapter review questions" in Kagan, Ozment, and Turner's *Western Heritage*. Published in 1995 by D. C. Heath, Lynn Hunt, Thomas Martin, Barbara Rosenwein, Po-Chia Hsia, and Bonnie Smith's *Challenge of the West* comes equipped with *A Student's Guide to Reading Maps, Interpreting Documents, and Preparing for Exams*, and *Instructor's Guide* with annotated chapter outlines and lecture suggestions, a video-disc whose 2,100 images are bar coded, captioned, and indexed for classroom use, a *Computerized Testing Program* and *Test Item File* of over 4,000 questions, and a transparency set. All cater to shorter tolerance levels, abbreviated attention spans, and students who, Prentice Hall's marketing manager reports, "are simply not reading the material assigned to them, whether in the textbooks or from primary sources."³⁵

That statement provides perspective about the plight of Western Civ as of other courses. Whatever we choose to call them, culture wars are as American as apple pie; curricular combat has always been a favorite academic sport; canons have evolved with perspective. If students don't read, does it matter that they don't read Toni Morrison rather than Pascal? Perhaps we should worry less about generations growing up ill indoctrinated and more about their growing up illiterate.

Newly fashionable notions, meanwhile, continue to be injected into revised editions like silicone, to amplify the original product and make it more appetizing. This too dates back to generations before illiteracy became a serious problem. Louis B. Snyder's *Making of Modern Man* (1967) already integrated documents with every chapter, added essays by "outstanding historians in the field," devoted serious attention to artistic and cultural activities as to psychological interpretations, included critical selections on imperialism. But its outstanding historians represented the grand old tradition (Becker, Salwyn Schapiro, Carlton Hayes), and the title itself would be unacceptable today. Wise production managers opt for less verbiage and more tricks. The 1976 edition of Wallbank's *Civilization* offered interchapter outlines, "profiles" of great men like Pascal or great topics like industrialism, color plates, maps, chronological charts, and a pronunciation key in the index providing the correct enunciation for all proper names of persons and places cited. Scott, Foresman were showing the way.

Production was not the only realm where authors were no longer left to work alone. Multiplicity of techniques called for multiplicity of skills in writing too. Generalists increasingly gave way to specialists working as a team, coordinated by production managers who were sometimes called editors. Collaborative world histories by many authors, like Lavis and Rambaud's *Histoire générale* (1893–1901) or the *Cambridge Modern History* (1902–12) were nothing new. One-volume team enterprises bringing together two, three, or five authors were a new phenomenon. In the 1950s, joint authorship became a marketing ploy, before it established itself as the norm. William Langer lent his name and scholarship to a coproduction of Harper & Row with *American Heritage*, responsible for the illustrations. Prentice Hall sold Brinton, Christopher, and Wolf's excellent history "from Plato to Pareto" on the strength of a distinguished trio of names. Then confederates and auxiliaries began to show up.

In the late 1970s Houghton Mifflin offered *A History of Western Society* by John P. McKay, Bennett Hill, and John Buckler: social history was pulling ahead of intellectual history. More "relevant," more approachable, and just as well written as the Brinton book, McKay's acknowledgments list twenty-nine names of instructors "who have read and critiqued the manuscript through its development." By 1995 Hunt et al.'s *Challenge of the West* acknowledged by name 269 colleagues "who have contributed formal written reviews," 8 more who presumably gave aid and comfort, and 10 production staff collaborating in "the development of this textbook." A book is no longer written; it is developed.

It sounds like a cumbersome process; but the multiadvisor, multiauthor text drives out the single author generalist. In 1955, Stewart Easton of the City College of New York finished *The Heritage of the Past, from the Earliest Times to 1500*, and Richard Brace of Northwestern University completed *The Making of the Modern World, from the Renaissance to the Present*. Published by Holt, Rinehart in 1960, these were model textbooks, challenging and beautifully written, especially that of Easton. Holt, Rinehart prints them no more; they are no longer to be found. In 1973, D. C. Heath in search of new approaches published F. Roy Willis's *Western Civilization: An Urban Perspective*: three volumes keying civilization to the rise of

the city and the process of urbanization. Twenty years later, Willis's urbane interlacing of cultural, political, and socioeconomic strands has been left to go out of print. It is not the publisher's fault: they print what sells, produce what courses will consume.

The vogue of World Civilization texts and courses is part of these adjustments to changed, or allegedly changing, demand. The Second World War spurred an interest in human rights extending beyond the Western Hemisphere. Colonial problems and decolonization, the self-assertion of colonial people and of blacks in America, the new scope of international relations publicized by penetrating media and by international bodies with a vested interest in global issues, the decline of Europe itself and the worldwide range of American interests, all suggested a new perspective. In the 1960s, just as UNESCO began to bring out its unwieldy *History of Mankind*, the Ford Foundation decided to encourage the study and teaching of history on a global scale. Supported by a Ford grant, Leften Stavrianos of Northwestern University, where the history department left freshmen free to choose between introductory courses in Western Civ, World Civ, or American History, published his *World since 1500: A Global History* (1966). "Traditional topics of European history," the preface declared, "are irrelevant for world history and must be discarded." In 1964, with a grant from the Carnegie Corporation, William McNeill of the University of Chicago began to write his *World History*, tried it out in an experimental course offered in 1964–65, and revised it by the end of 1966. Published in 1967 by the Oxford University Press, the book achieved a well-deserved *succès d'estime*. On the market, as one publisher put it, it proved a total bomb. Problems of compression, selection, interpretation, awesome enough in narrower fields, grew direr on a global scale; and the increasing abstraction that scale imposed went ill with the decreasing sophistication of the student body. Besides, since World Civ grew out of Western Civ (whether as extension or as reaction), it was condemned to use its language and to reflect its values even when it rejected them.

More traditional texts sometimes did better, when they adjusted to the new trend. The preface to the 1976 edition of Wallbank's *Civilization* spoke of space-ship earth and the global village, and claimed that until 1942, when the first edition of the book appeared, "no college text had surveyed the history of civilization as the joint achievement of all humanity." Long before Ford or Carnegie broached such notions, Wallbank and Taylor apparently set out to "present a survey of global cultural history treating the story of humanity not as a unique European experience but as a global one." They did, indeed, devote significant space to overseas cultures, to *négritude*, to Indian and Latin American literatures. But the global village remained a cultural colony of Europe, whose "influence spread to virtually all parts of the non-European world," and whose colonial enterprise (that interwar texts vigorously denounced) was recognized as an "instrument of cultural diffusion on a huge scale."

This is the great problem that world historians face: they flee Eurocentrism only to meet Europe in Samarra, where they are forced to recognize its leading role, especially "from about 1500 when the European Far West began to outstrip

the other major civilizations of the world.”³⁶ That is William McNeill, for whom the rise of the West to dominance over the globe constitutes the main theme of modern history. Why? Because “European inventiveness and ingenuity” contrasts with Chinese, Moslem, Hindu rejection of novelties.³⁷ Europe is ever transforming itself; others do not do so. Reluctant, retarded, or inert, non-Western dwellers in McNeill’s world aspire to Westernization or are simply destined to it, “since power, wealth, and truth itself beckon men everywhere to pursue industrialization, democratization, and science—all of them stemming directly from the Western past.”³⁸ Reasonable enough in the 1960s when they were formulated, such views ill fit the more demanding myths and aspirations of the 1990s. Though they may start out with exciting ideas, serious historians risk ending up politically incorrect when they write serious history.

That could be one of the reasons why the “stampede away from Western Civ” that John A. Scott observed in 1976³⁹ has slowed down since, and Western Civ texts today outsell World Civ by roughly two to one. What is true of texts is true of courses too. By 1985, most participants in the Michigan conference that led to the book *What Americans Should Know* “preferred a survey based on the history of Western Civilization.”⁴⁰ Despite what may be the wave of the future, declared Richard Sullivan with an eye on a wave of the past, “I suspect that the Western Civilization survey will be part of the academic landscape for a while. . . . Along with the American survey course, the Western Civilization survey strikes me as the only fixed and firm anchor the historical profession has in the chaos of modern education.”⁴¹ Edward Fox said the same thing differently: “no young citizen of the United States can find his true historical identity outside the context of the European past . . . our modern world was created largely by Europe.”⁴²

These people do not want Western Civ to remain unaltered, they submit a wealth of suggestions for improving the old course, but once again most of their (excellent) suggestions are strongly reminiscent of Robinson’s innovations or, at least, of their spirit. As Mr. Dooley observed: “I see great changes takin’ place in my day, but no change at all ivry fifty years.”

One of our day’s great changes is the tendency of some teachers to reject texts for sources, and to abandon collections of readings crammed with many shortish passages for a few integral documents like Rousseau’s *Social Contract*. There is plenty of room for debate on this (in the absence of text, what will furnish the context?), as there is in the proposed reforms of texts and syllabi criticized as too narrow, too blinkered, too exclusive or too contrived. But what William Bennett denounced in his 1992 book, *The De-valuing of America*, appears rather as a re-valuing, similar to reinterpretations that have never ceased since the inception of the course. Meanwhile, alternative courses designed to provide more self-esteem than learning compete with Western Civ but do not replace it.

Publishers so far have little reason to worry. Describing “The Decline and Rise of Western Civilization” in December 1982,⁴³ Karen Winkler reported the ninth edition of Burns’s *Western Civilizations* selling more copies in 1980 than in any previous version, Wallbank’s *Civilization* “selling better than ever,” and other contenders proving “remarkably successful.” From a national low of 200,000 or

300,000 students in the 1970s, Western Civ courses enrolled over 500,000 by the early 1980s. "Teachers are going back to the standard text. They are finding that students don't have the background in high school that allows them to follow the thematic approach."⁴⁴ Isn't that where we came in?

NOTES

1. Gilbert Allardyce, "Rise and Fall of the Western Civilization Course," *American Historical Review*, June 1982, pp. 695, 725.
2. John Erskine, *My Life as a Teacher* (Philadelphia, 1948), p. 26.
3. Annual Report of the American Historical Association for 1906 (Washington, D.C., 1907), p. 122, quoted in Allardyce, "Rise and Fall," p. 702.
4. Erskine, *My Life*, p. 154.
5. See *New York Times*, July 1, 1960, November 30, 1964, May 2, 1976, and, most recently, August 18, 1994.
6. Christine Naitove and Barbara Bartle in *AHA Perspectives*, June 1983, pp. 16–21.
7. Dean Acheson on the North Atlantic Pact in Eugen Weber, *The Western Tradition*, II (Lexington, 1995), p. 666.
8. Carol Gruber, *Mars and Minerva* (Baton Rouge, 1975). See also Committee of Seven, *The Study of History in Schools* (New York, 1899), p. 18: history meant to prepare students "for social adaptation and for forceful participation in civic activities."
9. Columbia University, *An Introduction to Contemporary Civilization in the West* (Columbia, 1930), assignment one.
10. *Ibid.*, p. 4.
11. John Erskine, *The Memory of Certain Persons* (Philadelphia, 1947), p. 206.
12. Erskine, *My Life*, pp. 165–71.
13. See Richard Popp, ed., *Teaching Western Civ at Chicago* (Chicago, 1987), n.p.
14. Hayward Keniston, Ferdinand Schevill, and Arthur P. Scott, eds., *Introductory General Course in the Humanities: Syllabus* (Chicago, 1931 ff), pp. 338–39.
15. *Autobiography of Andrew Dickson White* (New York, 1905), p. 42.
16. A. D. White, *A History of the Warfare of Science with Theology in Christendom*, II (New York, 1903), pp. 393–96.
17. Karl Lamprecht, *What Is History?* (New York, 1905).
18. Krieger in John Higham, Leonard Krieger, and Felix Gilbert, *History* (New York, 1965), p. 258.
19. Krieger, *ibid.*, p. 267. For Robinson, see Luther V. Hendricks, *James Harvey Robinson, Teacher of History* (New York, 1946), p. 106.
20. J. H. Robinson, *The New History* (New York, 1912), pp. 135–37.
21. "The New Allies of History," read to AHA in December 1910, reprinted in *ibid.*, p. 83.
22. C. J. H. Hayes, *A Political and Cultural History of Modern Europe*, I (New York, 1936), p. vii. Interestingly, at practically the same time, the Introductory General Course in the Humanities at Chicago was described as essentially a course in "our ruling Western Civilization . . . our own Western Civilization . . .," C. S. Boucher, *The Chicago College Plan* (Chicago, 1935), p. 44.
23. Columbia University, *Introduction to Contemporary Civilization*, p. 352.
24. Harry Elmer Barnes, *History of Western Civilization* (New York, 1935), p. viii; Barnes,

Intellectual and Cultural History of the Western World (New York, 1937; revised 1941), pp. 1116–17, 1214–15.

25. Burleigh Taylor Wilkins, *Carl Becker* (Cambridge, 1961), p. 152.

26. *Ibid.*, pp. 172–73; Charlotte Watkins Smith, *Carl Becker: On History* (Ithaca, 1956), p. 11.

27. I am grateful to Don Lamm, president of W. W. Norton, and to the publishers for opening access to the Norton papers in the Department of Special Collections, Columbia University Library.

28. Wallace Ferguson and Geoffrey Bruun, *Survey of European Civilization*, 1 [to 1660] (Boston, 1936), preface.

29. At Harvard, Western Civilization first appears as a course title in 1946, with Crane Brinton's Introduction to the Social Inheritance of Western Civilization.

30. George Mosse, "Freshman History," *Social Studies*, March 1949, p. 3. First at Iowa, then at Wisconsin, Mosse was one of the legendary presenters of Western Civilization.

31. Allardyce, "Rise and Fall," p. 715.

32. Josef Konvitz, ed., *What Americans Should Know: Western Civilization or World History?* (Lansing, 1985).

33. Carolyn Lougee, "Comments" on Allardyce, in *American Historical Review*, June 1982, p. 729.

34. Abby Kleinbaum, "Women's History and the Western Civilization Survey," *History Teacher* (1979).

35. Alison Prendergast, marketing manager, history, for Prentice Hall, in a circular addressed to history teachers, designed to publicize the fifth edition of Kagan et al.'s *Western Heritage*.

36. William McNeill, *A World History* (New York, 1967), p. 416.

37. *Ibid.*, p. 286.

38. *Ibid.*, p. 437.

39. John A. Scott, "Invited Comment" on Beyond Western Civilization symposium, in *History Teacher*, August 1977, p. 538.

40. Konvitz, *What Americans Should Know*, p. 6. An original founder of the present Western Civilization course at Chicago, McNeill's attempts to replace Western Civ with World Civ there seem to have met scant success. Meanwhile at UCLA, where students can choose between the two courses, present enrollments in World Civilization courses tally round one-fifth or one-sixth of those in Western Civ.

41. *Ibid.*, p. 264.

42. *Ibid.*, p. 205.

43. Karen Winkler, "Textbooks: The Decline and Rise of Western Civilization," *Chronicle of Higher Education*, December 1, 1982, pp. 23–24.

44. *Ibid.*

American Classical Historiography

RICHARD SALLER

THE HISTORIOGRAPHY of ancient Greece and Rome presents special problems in meeting the aim of this volume—that is, to explore the *distinctive* character of professional historical scholarship as it developed in the United States since the late nineteenth century. Classics as a field is especially transnational in character, based on a notion of a common European cultural heritage. Classical historians are expected to read publications written in French, German, or Italian, and a recent conference on the city-state in classical antiquity and medieval Italy pointedly eschewed translation of the conference papers into English for publication.¹ Through the twentieth century, there has been significant circulation of classical historians between the United States and Europe. Before World War I, many American classicists went to Germany for their doctoral research. Over the past twenty-five years, ancient historians educated in England have moved to the United States to occupy the majority of senior positions in classical history at major American research universities. This movement across national boundaries makes it difficult to isolate the distinctively American contribution.

To illustrate the difficulty, two of the towering figures in classical history in the twentieth century, M. I. Rostovtzeff and A. Momigliano, held appointments at American universities, but I cannot see how the intellectual formation of either man could be characterized as significantly American. Momigliano said of Rostovtzeff that he would not have written his great *Social and Economic History of the Roman Empire* (1926) if he had not left Russia in exile. That may be true in the sense that his exile to the United States was an important stimulus, but that is not to say that the work shows particularly American intellectual influences. The book was written soon after his arrival in the United States in 1920 and was marked by his Russian experience.² As for Momigliano, it has been suggested that he was more willing later in life to write about his Jewish heritage in an American milieu, but he was certainly writing about it before he took up the Whiting Visiting Professorship at the University of Chicago in 1974. As his colleague there, I would say that he did not convey the impression of intellectual assimilation to the United States.

To clarify the project and to keep it within manageable bounds, it seems sensible initially to define “distinctively American” rather narrowly by setting aside these problematic cases. In limiting my focus, I will admittedly pass over some of the most distinguished cosmopolitan scholarship in the field. To name only two

major contemporary Roman historians at American institutions, I omit Peter Brown and Glen Bowersock on the grounds that, though they have made landmark contributions to late imperial history, I am unable to define qualities of their work that are *distinctively* American. My essay will trace the following themes: (I) the “native American” pragmatic thread of classical historiography; (II) the American rejection of, or disinterest in, Marxist approaches to classical history; and (III) classical historians’ involvement in current American political controversies.

I. Chester Starr, one of the founders and first president of the American Association of Ancient Historians, recently reviewed the field of ancient history in the United States and pointed to what he believed to be *distinctively* American. American ancient historians are “far less inclined than their French or English peers to apply seriously concepts from anthropology or sociology; their analyses are much less theoretical than say Finley in *The Ancient Economy* or, better, *Politics in the Ancient World*. One might sum up the distinction by labeling American scholarship as pragmatic, a source of optimistic strength if also limiting the free play of imagination.”³ Setting aside M. I. Finley for the moment, I believe Starr is right to suggest that American classical historians’ self-identity has been marked by a pragmatic, antitheoretical streak. The works of Tenney Frank and Lily Ross Taylor, the two most distinguished “native American” Roman historians of the earlier twentieth century, illustrate how this self-identity was manifested.

Tenney Frank is interesting for my purposes because he offers a direct comparison with Rostovtzeff. Frank (1876–1939; Ph.D., University of Chicago, 1903) published two major books in the second decade of the twentieth century: *Roman Imperialism* (1914) and *Economic History of Rome* (1920). Shortly after the latter appeared, it was put in the shade by Rostovtzeff’s monumental *Social and Economic History of the Roman Empire* (1926). One year later Frank published a revised edition of his *Economic History*, which responded to Rostovtzeff’s book. In the revision, we can see him defining his American understanding of Roman history against Rostovtzeff’s understanding.

Frank took the view that American historians occupied a specially privileged position from which to understand the Roman republic. It was superior, in his mind, insofar as it was not distorted by oppressive European traditions with all their rigidities. The preface to his *Roman Imperialism* (1914), which argues the thesis that Roman expansion was a matter of defensive responses to others’ aggression, makes this explicit: whereas “old-world political traditions have taught historians to accept territorial expansion as a matter of course,” Frank as an American was able to discard that false premise and so more truly interpret Roman (and American) behavior.⁴ Jerzy Linderski has elegantly demonstrated how “Tenney Frank’s Rome was a mirror image of the America of McKinley and Theodore Roosevelt,”⁵ but for Frank it was a matter of getting rid of the blinkers of Old World tradition. Frank’s underlying assumptions of the unnatural distortions of the Old World are perhaps most starkly stated in his chapter on Egypt in

the *Economic History of Rome*, where he notes the peculiarity of Egypt: "Nature long ago eliminated normal society in Egypt."⁶ What did he mean by the disappearance of "normal society"? "The river imposed autocracy and servility as the price of existence. Individualism was out of place. . . ." By implication, he as an American was better able to appreciate the "natural"—that is, "individualistic"—than Europeans of his own day, who still lived within the "rigid class system of modern European states."⁷

It was Frank's view that Americans were especially well placed to understand the early Romans of the republic, because they were kindred spirits. The early Romans of Frank's *Economic History* were orderly, pragmatic agriculturalists, who from the beginning had a healthy respect for private property rights. (In fact, the "beginning" is wholly unattested by any evidence.) The respect for private property began after the Romans took land away from the early natives of Italy, but "it is not likely that the savages who were there before contested possession with any vigor. Peoples who use land chiefly as hunting ground do not risk enslavement or death in the defense of their lands."⁸ In other words, the early Romans, like frontier Americans of Frank's own day, were justified in taking land from "savage" natives on the grounds that it was not fully utilized. Under Roman governance, Italy and the rest of the empire prospered through the second century after Christ under a policy of *laissez-faire* in accordance with natural individual liberty. "Economic and social *laissez-faire* has never been more consistently practiced [than under Augustus]. After all, it was probably the quickest road to success if he really cared for Romanization. Peace through the empire gave the opportunity for material development to those who desired it, and prosperity brought satisfaction and goodwill towards the government, which in turn invited closer relations and a natural assimilation of Roman customs."⁹ In the Romans, then, Frank found the antecedents of American liberalism.¹⁰

As an American who naturally had a direct intuition of the truth about the Roman world, Frank had no need of conceptual sophistication. Instead of theory, Frank possessed American pragmatic common sense, as evidenced in his proverbs and his racism.¹¹ He explained the lack of commercial development in Rome in these terms: "Necessity, the mother of crafts as well as of arts, never forced [the Romans] into apprenticeship in those occupations that develop the love for artificial beauty and train the instincts for commercial enterprise."¹² The "instincts" were based on race, and changes of racial composition were the most basic cause of the major events of Roman history, including the fall of the empire. "The irresistible determination, the power of self-control, the stolid puritanism, as well as the hardness and self-sufficiency of the native old Roman were racial qualities, a part of the blood inheritance transmitted after the centuries of hard-handed struggle had weeded out the unfit."¹³ As race accounted for Rome's success, so also it accounted for its decline. "It would be interesting to know how far the social transmutation we have tried to follow [i.e., the migration of slaves into Italy] accounts for the fundamental changes in the Empire. Was not absolutism inevitable when the Italian, who had so equably combined liberty with law, gave

way to impulsive and passionate races that had never known self-government?"¹⁴ Whereas Rostovtzeff narrated the decline of the Roman Empire in terms of class conflict (a revolt of the peasant class against the urban bourgeoisie), Frank told a story of the contamination of the hardy stock of Roman individualists. The contrast captures the difference between the Continental interpretation and the American.

Lily Ross Taylor, Frank's most distinguished student, took her Ph.D. at Bryn Mawr in 1912, and then after fifteen years at Vassar College returned to Bryn Mawr where she taught until her retirement in 1952. Her work shares certain characteristics with Frank's. Like him, she believed in the possibility of a kind of unmediated, direct understanding of Roman antiquity. Her colleague, T. R. S. Broughton, notes that her first visit to Rome in 1909 "aroused her lifelong love of Rome," and he remembers her saying that "my aim as a teacher is to make my students feel that they are walking the streets of Rome, and seeing and thinking what Romans saw and thought."¹⁵ This is not an intellectual ambition that places value on finding an appropriate theory or developing an original method of analysis. With hindsight, we can see the anachronisms that point to the naïveté of the hope of direct experience motivated by a sentimental attachment to the classics.

Like Frank, Taylor had a limited appreciation of ancient Rome. As long as the Romans (like Americans) acted out their love of freedom during the republic, they were worth studying. But after the Romans degenerated into servility in the later decades of the empire, Taylor lost interest: "I have no interest in cataloguing the forms of flattery. . . . I abandoned the study of ruler cult when it was in danger of affecting my sanity."¹⁶ In other words, she had no interest in understanding deep cultural differences, nor did she have any method for such an understanding. In her account of the development of emperor cult in *The Divinity of the Roman Emperor* (1931) she distinguished between the Italians, who had better sense than to worship a living man and so worshipped Augustus' Genius, and the eastern subjects of the Roman Empire, who had a long tradition of gullible servility. Recent research has shown that this distinction between west and east is bogus—a result of Taylor's reluctance to admit that the freedom-loving Romans so quickly accepted the deification of a living man.¹⁷ Nevertheless, it has been the standard view for more than half a century.

Of Taylor's other books, the two on the voting assemblies of the Roman republic have been perhaps her most useful and enduring contributions, and were said by Broughton to illustrate her American pragmatic interest in how institutions work.¹⁸ Her mostly widely read book, *Party Politics in the Age of Caesar* (1949), is not peculiarly American insofar as it follows Sir Ronald Syme's *Roman Revolution* (1939) in understanding Roman politics in terms of personal alliances of kinship and friendship among the aristocracy. In its focus on the political elite, Taylor's *Party Politics* is representative of the mainstream of American scholarship on Roman political history, illustrated more recently by Erich Gruen's *Roman Politics and the Criminal Courts, 149–78 B.C.* (1968) and *The Last Generation of the Roman Republic* (1974). A counterpoint may be found in Ramsay MacMullen's work,

including *Enemies of the Roman Order* (1966) and *Roman Social Relations, 50 B.C. to A.D. 284* (1974), which extend the historical horizon to the lower classes, rural laborers, and social outcasts of the Roman Empire.

II. The rise of fascism in Europe provoked migrations that threatened to inject more sophisticated social theory into American classical historiography, in particular through the mediation of Moses Finley. It is of course ironic that Starr's exemplar of European theory in classical history is Moses Finley, who was born and educated in the United States (b. 1912; B.A., Syracuse University, 1927). Studying at Columbia University with W. L. Westermann in the 1930s, Finley came into contact with the Frankfurt school in exile. His reviews from the 1930s were published in the school's *Zeitschrift für Sozialforschung* and show the influence of Marxist and other sociological thought. World War II brought a long hiatus in Finley's publications. As his major works began to appear in the early 1950s, he was caught up in the McCarthy purges in American universities. Though no evidence was produced to prove that he belonged to the Communist party, he refused to testify before the Senate Subcommittee on Internal Security. As a result, he was fired from Rutgers University in 1952. Blackballed and unable to find another position for two years, Finley emigrated from the United States to England, where A. H. M. Jones secured a fellowship for him at Jesus College, Cambridge, in 1954. By this time, Finley was more Weberian than Marxist, but his past association with Marxists and his unwillingness to name names before congressional committees were sufficient grounds for de facto condemnation.¹⁹

Though Finley did not take a Marxist approach to classical history at the height of his career, he did insist that Marxism be taken seriously. With the loss of his voice, the politics of classical historiography in the United States took a different course from that in Europe. To simplify, the politics in European classical historiography was organized around the axis of Marxism and its critics.²⁰ In the Marxist historical-developmental scheme, of course, classical antiquity was characterized as dominated by the slave mode of production; consequently, the significance of slavery in antiquity became a central issue in the debate.²¹ As late as the 1980s, fierce arguments over the explanatory value of the slave mode of production for the decline of the Roman Empire were going on in England and on the Continent. In England the focal point was G. E. M. de Ste Croix's *Class Struggle in the Ancient Greek World* (1981). On the Continent, the Gramsci Institute held a major conference on the slave mode of production in Pisa in 1979, which resulted in a three-volume publication entitled *Società Romana e Produzione Schiavistica* including papers by numerous Continental and British scholars, but no American classical historian.²²

American classical historians generally absented themselves from the argument over Marxism and slavery. Finley's teacher at Columbia, W. L. Westermann, published in 1955 the major synthetic treatment in English of ancient slavery, *The Slave Systems of Greek and Roman Antiquity*, but the book takes a positivist approach that yields a collection of evidence, not organized by a Marxist or any other problematic (with the exception of the two chapters showing that

Stoicism and Christianity cannot take credit for the decline of slavery in the Roman Empire). Despite the importance of the subject, the book is disappointing for its lack of social or economic analysis.

Starr, a successor to Frank and Taylor in taking an "American pragmatic" approach to classical history, published an article in 1958 indicating why American classical historians did not take seriously the debate over the Marxist interpretation of slavery. In "An Overdose of Slavery," Starr urged "that one view ancient slavery without the blinkers of nineteenth-century humanitarianism or twentieth-century Marxist totalitarianism."²³ In a bare fifteen pages the article sets out to demolish some of the more extravagant claims of "British socialist historians" such as George Thomson and Benjamin Farrington, but it does not show an understanding of the issue of the location of slavery in the ancient social structure. Starr believed that he had undermined the Marxist interpretation with the assertion that the proportion of slaves in the classical Athenian population was "probably one third or one quarter *at most*" (that is, as large a proportion of the population as in the antebellum American South) and concluded that "one must seriously question modern attempts to derive *all* ancient immorality" from slavery.²⁴ No citation indicates what Marxist or humanitarian historian had claimed that slavery was responsible for *all* ancient immorality: Starr's simple conceptualization made his conclusion both obvious and useless. Not all European classical historians were persuaded that they suffered from "a bad overdose of slavery," and the controversy over the structural significance of slavery continued without much American contribution.²⁵ Lacking an obvious theoretical overlay, the books and articles of Ramsay MacMullen about the lower classes mentioned above did not directly participate in this debate.

III. In the United States contemporary political issues other than Marxism—especially problems of minority rights—have stimulated the most heated debates among classicists, which have received attention in the popular press.

In the 1960s and 1970s the war in Vietnam stood at the center of the political storms in the United States. The parallel between American intervention in Southeast Asia and Roman wars of expansion seemed apparent to some ancient historians: like Rome, the United States intervened far from its borders on the pretext of a local invitation, but in reality on account of its own amoral, material interests. In the wake of the Vietnam War were published two major studies of Roman imperialism by historians in the United States: W. V. Harris's *War and Imperialism in Republican Rome, 327–70 B.C.* (1979) and Erich Gruen's *The Hellenistic World and the Coming of Rome* (1984).²⁶ The debate over Roman motives for expansion was international, but the Vietnam War lent a special intensity to the arguments in these books and in classrooms around the country. An evaluation of a Roman history course written by an American student in the early 1970s bluntly protested that the professor should "get the Romans out of Vietnam."

Out of the civil rights movement has grown a variety of rights movements in American politics. The classicists' professional association, the American Philological Association, has provided a small stage for battles over women's rights,

lesbian and gay rights, and multicultural studies. Among its other interest groups, the APA includes the Women's Classical Caucus and the Lesbian and Gay Caucus, which have occasionally struggled over the program and the venue of the annual meeting. The tension may be judged from the fact that several years ago the officers of the APA retained legal counsel to advise in their programmatic deliberations, and they acquired insurance for directors and officers to cover legal liability.

In the field of women's studies, Sarah Pomeroy's *Goddesses, Whores, Wives, and Slaves: Women in Classical Antiquity*, published in 1975, was a landmark for its historical synthesis of women's experience in Greco-Roman antiquity. The book, now translated into numerous languages, stressed the fundamental subordination of women. Pomeroy's was certainly not the first treatment of Greek or Roman women. A decade earlier J. P. V. D. Balsdon had written a book about Roman women in which he recognized the methodological problem of understanding women's experience through sources overwhelmingly dominated by male voices.²⁷ For Balsdon this meant that we hear more husbands' complaints about wives than wives' complaints about husbands, and he accepted the evidence of Roman imperial funerary inscriptions, which "record the uneventful happy marriages of thousands and thousands of ordinary men and women."²⁸ By contrast, Pomeroy (rightly) emphasized the inherently hierarchical nature of the (happy or unhappy) conjugal bond in Greek and Roman antiquity.

The reviews of *Goddesses, Whores, Wives, and Slaves* from the male academic establishment generally praised it for its range and grasp of the evidence, while noting that it was better regarding Greek women than Roman.²⁹ It is interesting that some of the most pointed criticism came not from male classicists but from a distinguished historian of American women, Anne Firor Scott. She identified in the book much the same fault that had been found in other works of classical social history written by Americans—that is, the lack of a clear analytical or theoretical framework or major historical questions: "There are an enormous number of 'facts' somewhat jumbled together. . . . There are indeed times when the author appears to believe that all facts are created equal."³⁰ This critique, I believe, represents an attitude widely held by American historians of later eras toward classical history, which they see as learned but out of touch with substantive and methodological interests of the discipline of history. Whatever the conceptual problems of Pomeroy's book, it has been important as a textbook, which (along with the Lefkowitz and Fant source collection, *Women's Life in Greece and Rome*) has enabled the curricular innovation of offering courses on women in the ancient world for thousands of American undergraduates.

Since publication of Pomeroy's book, women's studies have assumed a far greater importance in the classical field. Some of the work is particularly informed by feminist theory, but that influence is more evident in literary studies (for example, H. Foley, ed., *Reflections of Women in Antiquity*) than among works by those trained as Greco-Roman historians. For instance, the book on Roman marriage by the Oxford-trained historian Susan Treggiari of Stanford Univer-

sity presents a magisterial survey of the evidence, but without any particular theoretical inclination. In fact, it barely notices the controversy aroused by Foucault's *History of Sexuality*, though its conclusions have a direct bearing on that controversy.³¹

The impact of feminism on the mainstream of American classical historiography has been limited. I do not believe that any classical historian at one of the top five research universities in the United States today would describe herself or himself as motivated primarily by feminist theory. (There are very few women at all in such positions.) Nevertheless, in contemporary *literary* studies the efforts to understand the construction of gender categories are having an impact on the work of social historians. At the most recent meeting of the APA, the Women's Classical Caucus sponsored a panel entitled Women and Slaves in Classical Studies. The papers on gender and social categories in Greek epic and tragedy and Roman comedy were written by young scholars trained in literary criticism, but could just as well have been labeled "cultural history." Because the standard sources for classical history are so heavily literary, the field is especially susceptible to the blurring of the line between sociocultural history and literary criticism. In my view, that blurring can be beneficial in furthering our understanding of Greco-Roman culture, but it can also lead to a lack of the rigor regarding chronology and evidence that has traditionally been valued in the historian's work.

The history of women in antiquity has been only one site of a feminist debate of much wider historical dimensions, and I do not believe that classical historiography has had a leading or distinctive role in the argument. By contrast, the classical world has had a special place in the contest over lesbian and gay rights. Foucault's radical historicization of sexual morality in *The History of Sexuality* started from the Greco-Roman experience and has had a deep influence on classical scholars. Sir Kenneth Dover had already shown in *Greek Homosexuality* (1976) that classical Greek attitudes differed markedly from later Christian condemnation. The Yale medieval historian John Boswell then advanced the more sweeping claim in *Christianity, Social Tolerance and Homosexuality* (1980) that antipathy toward homosexual behavior in western European history began only in the thirteenth century of the Christian era. It followed that the existence of a prior era before repression opened the possibility of a return to a more tolerant, happier time, because attitudes toward homosexual relations are no more than arbitrary cultural constructs. Furthermore, repression of homosexuality was the historical exception rather than the rule. As a review of Boswell's book in the popular press noted, "to characterize this analysis as revolutionary—in its implications for historical studies, for Christianity, for the current debate over sexual mores—is to state the obvious."³²

Ramsay MacMullen, a Roman social historian also at Yale, quickly responded with a critique arguing that pre-Christian, Roman mores expressed strong disapproval of homosexual behavior, active and passive. Though homosexual behavior certainly existed, MacMullen argued, it was regarded with suspicion as a Greek import.³³ Later, in a major article and a book David Cohen, a Greek

historian at Berkeley, demonstrated that even in classical Greece the moral valuation of homosexual relations was contested, as some Greeks claimed that some homoerotic behavior represented an act of hubris against the passive member of the relationship.³⁴

Cohen's article in *Past & Present* led to a scholarly debate in that journal, but a far larger uproar arose several years later from the Colorado case known as *Romer v. Evans*. In 1992 the voters of Colorado approved an amendment to the state constitution forbidding special protection against discrimination on the basis of sexual orientation. The amendment was challenged as unconstitutional in the courts. Both sides in the case appealed to classical antiquity in the dispute over whether homosexuals constituted a traditionally "suspect class," and whether disapproval of homosexuality stemmed historically from "one narrow strand of a single religious tradition."³⁵ If the latter is true, then it could be argued that the Colorado amendment represented an enforcement of a particular religious sect's morality. John Boswell and Martha Nussbaum were summoned as expert witnesses to testify for the plaintiffs that the disapproval was based on "a sectarian Catholic argument" and that before Christianity same-sex relationships were not regarded as "indecent or immoral" or "shameful." Ramsay MacMullen was asked by the defense to submit an affidavit to refute these claims and showed that Boswell and Nussbaum misrepresented classical authors in order to substantiate their point and to suppress the truth. MacMullen concluded his affidavit with a strong accusation that it would be reasonable to infer that we find in Boswell's testimony "a sort of high treason in research, . . . deliberate deceit—the intent to fool people."³⁶ Only on this occasion have I received from a colleague a legal affidavit circulated as an offprint.

If the charges and countercharges have provided fuel for the bitter battles over political correctness and intellectual integrity, it is not obvious that they produced any substantial conclusions relevant to the Colorado case: Judge Bayless's decision noted (wearily?) "that the plaintiffs filled the witness stand with doctors, psychiatrists, genetic explorers, historians, philosophers, and political scientists," but he apparently found no compelling relevance of classical antiquity to the legal issues at hand.³⁷ The contentious Colorado case allowed classical scholars to claim a share of the public spotlight with the assertion that ancient practices and values are relevant to contemporary issues: unfortunately, the outcome has raised more questions about academic integrity than it has resolved about the history of sexual mores.³⁸

The debate has reemerged with its bitter edge in the press in recent months with the publication of Boswell's final book, *Same-Sex Unions in Premodern Europe* (1994). A similar pattern of exchange is evident, with charges of quoting out of context and countercharges of lack of linguistic expertise.³⁹ Classical history will remain a specially charged field for debate of these issues, owing to the importance attached to pre-Christian moral values as a baseline from which to judge the peculiarity of Catholic doctrine condemning homosexual behavior in a nation that attempts to maintain a separation between church and state.

The politics of gender studies in classical history has another dimension in the conflict between the feminists and the gay-lesbian group. The latter's claim of originality in historicizing gender categories has drawn an angry response from some feminist classicists like Amy Richlin, who believes that feminist scholarship has gone unnoticed or uncredited by gay classicists, as well as by their traditional male colleagues. Neglect of women's experience is rooted in Foucault's lack of interest in the *History of Sexuality*: even as he argued that symmetry in the marital bond developed during the Roman Empire, his focus was overwhelmingly on the subjective experience of self-fashioning males.⁴⁰ This has been a bitter pill for some feminists to swallow, as Richlin points out, since "feminists, historians of sexuality, and theorists should be natural allies; certainly they are lumped together by the right."⁴¹

A third area in which classical historiography has entered contemporary political controversy is the debate over the privileged position of European culture in a multicultural American nation. The complexities of the debate defy brief summary, but the reception of Martin Bernal's *Black Athena* among classical historians deserves comment. An expert in Chinese politics, Bernal set out to challenge the authoritative position of classical Greece as the foundation of European civilization by arguing that "Greek culture had arisen as the result of colonization, around 1500 B.C., by Egyptians and Phoenicians who had civilized the native inhabitants."⁴² Bernal claims that the Greeks themselves believed this to be true, and that the contemporary understanding of Greek culture as primarily Indo-European is a figment of "Northern European racism of the nineteenth century."⁴³ As for the consequences of this position, "if I am right in urging the overthrow of the Aryan Model and its replacement by the Revised Ancient one, it will be necessary not only to rethink the fundamental bases of 'Western Civilization' but also to recognize the penetration of racism and 'continental chauvinism' into all our historiography, or philosophy of writing history."⁴⁴

Despite its confrontational posture, *Black Athena* was initially received favorably by some senior classicists, who were persuaded by Bernal's intellectual history of the construction of the "Aryan Model."⁴⁵ However, his treatment of the ancient linguistic and archeological evidence, especially in the second volume, has been read with increasing skepticism by classicists and Near Eastern specialists alike. On closer inspection, it has been pointed out, classical scholarship had already been moving toward a recognition of Near Eastern influences on Aegean civilization. Three decades before publication of *Black Athena*, Moses Finley had already placed the Mycenaean palaces of the late Bronze Age at the periphery of the world of redistributive temple economies of the Near East.⁴⁶ Bernal's specific claim of second-millennium colonization of Greece by the Egyptians has generally been resisted by specialists as unsubstantiated by archaeology. Nor have scholars been willing to follow Bernal in placing greater confidence in the ancient legends reported by Herodotus than in the continuity of the material remains. More fundamentally, archaeologists and anthropologists find Bernal's unidirectional diffusionist theory outdated: they are willing to accept the idea of cultural

exchange in the eastern Mediterranean of the second millennium, but not to assume that the influence moved in only one direction. Bernal is trapped in the same illusory search for proprietary origins as those he criticized.⁴⁷

The discussion of *Black Athena* has been international, but with a special political edge in the United States, where it has become part of the debate over Afrocentrism and Western Civilization.⁴⁸ In becoming a lightning rod in the battle over multiculturalism, the argument over *Black Athena* regarding cultural influences in the eastern Mediterranean has been polarized, distorted, and confused. Behind the resistance to his rewriting of history, Bernal sees European racism and cultural arrogance; critics of Bernal respond that he, like earlier Afrocentrists, is trying to avoid the common critical standards of scholarly debate (for example, in his selective acceptance of the historical veracity of Greek myths and legends).⁴⁹

The three political rights movements—feminism, the gay/lesbian activism, Afrocentrism—have inspired new histories that have challenged the standard narrative of America's European cultural heritage in different ways. Feminist classical historiography has pressed for expanding the circle of historically significant actors to include women. Though this inclusion does not necessarily challenge the privileged place of Europe in Americans' historical consciousness, it is at least indirectly subversive insofar as it denies the traditional criteria of historical significance that emphasized power as embodied in (male) politics and war. Through this shift of focus feminist historiography may also directly challenge the traditional historical valuations in the American historical curriculum: for instance, should Western Civilization courses celebrate Athenian democracy, if that democracy excluded and oppressed women (and slaves)? In contrast, the lesbian and gay classicists seem happy to privilege and celebrate classical Greco-Roman antiquity as an era (they claim) before the tyranny of sexual inhibitions imposed by the Christian Church. The Afrocentrists have aimed to deflate the pretensions of classical Europe, and then to claim credit for European culture embodied in classical Greece. Against these politically inspired movements, a political opposition has formed in the National Association of Scholars under the banner of the protection of scholarly standards. Its members have been part of the wider political debate about standards for the history curriculum in American high schools, but both the association and its opponents have had relatively little impact on the scholarship of professional circles of both established and younger classical historians.

Established classical historians in the United States have in the past decade produced important books about the nature of Greek democracy.⁵⁰ These works have participated in an international discussion about how democratic ancient Athens was and how its political institutions worked. The intense interest in democracy flows in part from the contemporary concerns of political theory about the participation of individuals in electoral processes.⁵¹ Although the interest in the Athenian experience with democracy certainly cannot be claimed to be distinctively American, it is worth pointing out that it represents a change from the interest of the American founding fathers in the classical world. The Federal-

ists among them generally condemned ancient Athens as an example of corruption; even among "the more democratically inclined" anti-Federalists, "the handful of references . . . to classical Athens were either negative or neutral."⁵² For the Federalists, republican Rome was more relevant as a political model of mixed government that limited the power of the masses.⁵³ Today, the electoral assemblies and the senate of republican Rome seem too different from contemporary American institutions to make them interesting as models for discussion.⁵⁴ In an age of mass media, the lack of mediation between politician and voter in Athenian democracy makes it appear more relevant. In any case, this shift in interest of political theory from Roman republicanism to Athenian democracy, like so many other aspects of classical historiography, has been transnational, not peculiarly American.

The truly powerful resistance to the challenge posed by the new women's, gay and lesbian, and Afrocentric histories comes not from the National Association of Scholars but from the inertia of "native American" classical historiography as displayed in the new generation of practitioners. A survey of titles of dissertations completed at major American research universities since 1990 suggests that the recent entrants into the profession are by and large traditional in their choice of subjects. If traditional is defined as conventional political or military history or historiography, then the traditional dissertations in classical history have outnumbered the nontraditional by two to one. In 1994 from the University of California at Berkeley, the University of Pennsylvania, and Harvard—three of the most respected and most active graduate institutions in classical history—came dissertations with the following titles: "Agathokles of Syracuse and the Greek West: The Coinage," "The Family of Konon and Timotheos," "The Political Biography of Gaius Gracchus," "Evidence of Interpolation in the Text of Thucydides," "The Rhodian Navy," and "Early Greek Democracies outside Athens."⁵⁵ These titles would not have been out of place at any time since the nineteenth century. Ramsay MacMullen may well be right that the resistance to broadening the subject of classical history is rooted in the philological training required in the doctoral programs, which concentrate on translating texts by elite male authors rather than exploring alternative methods to approach new problems.⁵⁶ I would add from my own experience that many of the students applying for graduate study in classical history seem to be self-selected for interests in traditional political and military history, which are often hard to change. The minority of less traditional dissertations today display an interest in rhetoric, symbolism, and ideology. The standard training in classical literature in American doctoral programs may serve classical historians well in developing these approaches.

American classical historiography has to be understood within the context of a transnational field of study, one in which the senior positions at American universities are held by foreign-born or foreign-trained scholars. As a result, much of the classical history written in the United States is indistinguishable from European or British scholarship, and I have not tried to review that corpus. Rather, I have attempted to delineate the identity adopted by American classical historians

in contrast to their European counterparts, and to show how that identity fit with more general notions of American exceptionalism among intellectuals in the early twentieth century. I have suggested how American classical historiography has been shaped recently by the more general political context. Since the Second World War Marxism has not been a strong force in the United States, and has not formed an urgent part of the intellectual agenda of American classical historians. Instead, political rights movements have provoked new histories. Those histories of women, gays and lesbians, and Africans in antiquity have not been peculiar to the United States, but the American political context has given them a controversial intensity and a special visibility in the American press, which is otherwise generally indifferent to classical scholarship.

NOTES

1. Anthony Molho, Kurt Raaflaub, and Julia Emlen, eds., *City States in Classical Antiquity and Medieval Italy* (Ann Arbor, 1991).

2. Arnaldo Momigliano, *Studies in Historiography* (New York, 1966), 92. Brent D. Shaw, "Under Russian Eyes," *Journal of Roman Studies* 82 (1992): 216–28, discusses the Russian and German intellectual influences on Rostovtzeff but does not mention the possibility of an American intellectual influence. As William Calder notes, Rostovtzeff's teaching at Yale produced students skilled in papyrology but none with his breadth of historical vision or methodological innovation ("Classical Scholarship in the United States: An Introductory Essay," in W. Briggs, Jr., ed., *Biographical Dictionary of North American Classicists* [Westport, Conn., 1994], xxxi).

3. Chester Starr, "Ancient History in the Twentieth Century," *Classical World* 84 (1991): 184.

4. Tenney Frank, *Roman Imperialism* (New York, 1914), vii.

5. Jerzy Linderski, "Si vis pacem, para bellum: Concepts of Defensive Imperialism," in W. Harris, ed., *The Imperialism of Mid-Republican Rome*, Papers and Monographs of the American Academy in Rome (Rome, 1984), vol. 29, p. 147.

6. Tenney Frank, *Economic History of Rome*, 2d ed. (Baltimore, 1927), 380.

7. *Ibid.*, 334.

8. *Ibid.*, 2.

9. *Ibid.*, 406.

10. Frank fits the broader pattern of liberal reaction of American exceptionalism to the historicist and structural intellectual currents of Europe described by Dorothy Ross, *The Origins of American Social Science* (New York, 1991), 346.

11. The wide acceptance of the explanatory power of race in American social sciences of this period is documented by Ross, *Origins of American Social Science*, 322 and passim.

12. Frank, *Economic History*, 67.

13. *Ibid.*, 124.

14. *Ibid.*, 217.

15. T. R. S. Broughton, "Lily Ross Taylor," in Ward W. Briggs, Jr., and William M. Calder III, eds., *Classical Scholarship: A Biographical Encyclopedia* (New York, 1990), 454.

16. *Ibid.*

17. Ittai Gradel, "Mamia's Dedication: Emperor and Genius. The Imperial Cult in Italy and the Genius Coloniae in Pompeii," *Analecta Romana Instituti Danici* 20 (1994): 45.

18. Broughton, "Lily Ross Taylor," regarding *The Voting Districts of the Roman Republic: The Thirty-Five Urban and Rural Tribes*, Papers and Monographs of the American Academy in Rome, vol. 20 (Rome, 1960), and *Roman Voting Assemblies from the Hannibalic War to the Dictatorship of Caesar* (Ann Arbor, 1966). At the conference Professor Emilio Gabba similarly characterized G. W. Botsford's earlier *The Roman Assemblies from the Origin to the End of the Republic* (New York, 1909).

19. See the editors' introduction to M. I. Finley, *Economy and Society in Ancient Greece*, ed. Brent D. Shaw and Richard P. Saller (New York, 1981), ix–xxvi.

20. For the powerful effect of Marxism on French intellectual life in postwar France, see Tony Judt, *Past Imperfect: French Intellectuals, 1944–1956* (Berkeley, 1992).

21. M. I. Finley, *Ancient Slavery and Modern Ideology* (New York, 1980), chap. 1. Much European classical historiography was directed toward technical questions without overt political tendencies.

22. A. Giardina and A. Schiavone, eds., *Società Romana e Produzione Schiavistica* (Rome-Bari, 1981), with the review article by D. Rathbone, "The Slave Mode of Production in Italy," *Journal of Roman Studies* 73 (1983): 160–68.

23. C. Starr, "An Overdose of Slavery," *Journal of Economic History* 18 (1958): 31.

24. *Ibid.*, 22, my italics; *ibid.*, 31, his italics.

25. A notable exception is Michael Jameson's "Agriculture and Slavery in Classical Athens," *Classical Journal* 72 (1977–78): 122–45.

26. Both books were published after the American withdrawal of troops from Vietnam in 1975, but both were started during the war. Ernst Badian's *Roman Imperialism in the Late Republic* (Ithaca, 1968) was written before his immigration to the United States and took an overtly anti-Marxist position in a more general debate about imperialism by colonial powers.

27. J. P. V. D. Balsdon, *Roman Women* (London, 1962), 212–14.

28. *Ibid.*, 206–7.

29. E. Badian, "The Lives of Ancient Women," *New York Review of Books*, Oct. 30, 1975, 28–31.

30. Anne Firor Scott, review of *Godesses, Whores, Wives, and Slaves*, in *History—Review of New Books* 4 (1975): 36.

31. Susan Treggiari, *Roman Marriage: Iusti Coniuges from the Time of Cicero to the Time of Ulpian* (Oxford, 1991).

32. M. B. Duberman, *New Republic*, Oct. 18, 1980, 33.

33. Ramsay MacMullen, "Roman Attitudes to Greek Love," *Historia* 31 (1982): 484–502.

34. David Cohen, *Law, Sexuality, and Society: The Enforcement of Morals in Classical Athens* (Cambridge, 1991), 182–83.

35. M. Nussbaum, affidavit 21 Oct. 1993 in *Romer v Evans*, para. 67, 18.

36. R. MacMullen, affidavit Oct. 1993 in *Romer v Evans*, para. 26.

37. *Romer v Evans*, 63 Empl. Prac. Dec. (CCH) 42,719 at 77,938 (Colo. Dist. Ct. Dec. 14, 1993). The philosophical witnesses were summoned by the plaintiffs to show that homosexuals are a traditionally "suspect class"—a claim which Judge Bayless denied.

38. In the aftermath of the initial legal hearings, both Nussbaum and her chief opponent, John Finnis of Oxford, have published long arguments and counterarguments in law journals, and have spoken on campuses across the country (Martha C. Nussbaum,

"Platonic Love and Colorado law: The Relevance of Ancient Greek Norms to Modern Sexual Controversies," *Virginia Law Review* 80 [1994]: 1515–1651; John M. Finnis, "Law, Morality, and 'Sexual Orientation,'" *Notre Dame Law Review* 69 [1994]: 1049–78). The results have been in some respects humorous and in other respects distressing. As an appendix to her 136-page article in the *Virginia Law Review*, Nussbaum tried to specify the qualifications for "expert witnesses" on classical subjects, including a sight-reading examination in Greek and Latin to be graded by Sir Hugh Lloyd-Jones or someone of comparable standing. More discreditable were Nussbaum's tactics to impugn the qualifications of certain scholars rather than to grapple with the substance of their arguments. For example, rather than address the arguments of Cohen's *Law, Sexuality, and Society*, Nussbaum (affidavit 21 Oct. 1993 in *Romer v. Evans*, para. 30) summarily dismissed Cohen because "he is not a classicist, . . . has never been employed by a department of Classics," but is a professor of rhetoric who is dependent on translations of ancient texts. In reality, Cohen has a Ph.D. in classics from the University of Cambridge, has a joint appointment in the Departments of Classics and Rhetoric at the University of California, Berkeley, and uses Greek texts in his book. The only element of truth in Nussbaum's dismissal is that Cohen is not a member of the American Philological Association, which is open to anyone ready to pay the membership fee. Among the other ironies of this testimony is the fact that Nussbaum herself no longer holds an appointment in a department of classics—a fact which presumably does not disqualify her from giving expert opinions in classical philosophy.

39. Brent Shaw's review in *New Republic*, July 18 and 25, 1994, 33–41, with reply by Ralph Hexter and counterreply by Brent Shaw in *New Republic*, Oct. 3, 1994, 39 ff.

40. David Cohen and Richard Saller, "Foucault on Sexuality in Greco-Roman Antiquity," in J. Goldstein, ed., *Foucault and the Writing of History* (Oxford, 1994), 35–60.

41. Amy Richlin, "Zeus and Metis: Foucault, Feminism, Classics," *Helios* 18 (1991): 177.

42. Martin Bernal, *Black Athena: The Afroasiatic Roots of Classical Civilization* (New Brunswick, 1987), 1.

43. *Ibid.*, xv.

44. *Ibid.*, 2.

45. G. Bowersock, review of *Black Athena*, *Journal of Interdisciplinary History* 19 (1989): 490–91; M. Vickers, review of *Black Athena* in *Antiquity* 61 (1987): 480.

46. Finley, *Economy and Society in Ancient Greece*, chap. 12; more recently, Walter Burkert, *The Orientalizing Revolution: Near Eastern Influence on Greek Culture in the Early Archaic Age*, trans. Walter Burkert and Margaret E. Pinder (Cambridge, Mass., 1992; German ed. orig. pub. 1984).

47. M. Shanks, review of *Black Athena* in *History Today* 42 (1992): 56.

48. S. Burstein, *Classical Philology* 88 (1993): 157–62, provides a balanced and knowledgeable account of the debate. Carol Thomas, *Myth Becomes History: Pre-Classical Greece* (Claremont, Ca., 1993), 54.

49. M. Lefkowitz, "Not out of Africa," *New Republic*, Feb. 10, 1992, 29–36.

50. For instance, Martin Ostwald, *From Popular Sovereignty to the Sovereignty of Law: Law, Society, and Politics in Fifth-Century Athens* (Berkeley, 1986), and Josiah Ober, *Mass and Elite in Democratic Athens: Rhetoric, Ideology, and the Power of the People* (Princeton, 1989).

51. M. I. Finley, *Democracy Ancient and Modern* (New Brunswick, 1973).

52. Jennifer Tolbert Roberts, *Athens on Trial: The Antidemocratic Tradition in Western Thought* (Princeton, 1994), 185.

53. Carl J. Richard, *The Founders and the Classics: Greece, Rome, and the American Enlightenment* (Cambridge, Mass., 1994), 53–84 and esp. 131.

54. The exception is the recent book of Paul A. Rahe, *Republics Ancient and Modern: Classical Republicanism and the American Revolution* (Chapel Hill, 1992).
55. *American Philological Association Newsletter*, June and Aug. 1994.
56. Ramsay MacMullen, "History in Classics," in P. Culham and L. Edmunds, eds., *Classics: A Discipline and Profession in Crisis?* (Lanham, Md., 1989).

In the Mirror's Eye

THE WRITING OF MEDIEVAL HISTORY

IN AMERICA

GABRIELLE M. SPIEGEL

THE TITLE of my essay—"In the Mirror's Eye"—derives both from the name of the most prominent journal dedicated to medieval studies in North America, the journal *Speculum*, edited and produced by the Medieval Academy of America, and from the image that adorns its cover. As E. K. Rand, the first editor, explained in the journal's inaugural issue, the choice of name was guided by the sense that

Speculum, this mirror to which we find it appropriate to give a Latin name, suggests the multitudinous mirrors in which people of the Middle Ages liked to gaze at themselves and other folk—mirrors of history and doctrine and morals, mirrors of princes and lovers and fools. We intend no conscious follies, but we recognize satire, humor and the joy of life as part of our aim. Art and beauty and poetry are a portion of our medieval heritage. Our contribution to the knowledge of those times must be scholarly, first of all, but scholarship must be arrayed, so far as possible, in a pleasing form.¹

For Rand and his cofounders of the academy and what they aspired to make its leading journal, medieval studies in America were thus consciously directed at overcoming the prejudices and ideological contamination that the very term "medieval" had acquired over the centuries, connoting a dark, backward, superstitious, "Gothic" age, what Karl Marx once called humankind's "zoology," or animal history. Instead, the image that the *speculum* of medievalism in America should display was, whatever its ultimate shape, above all, as Rand's prefatory remarks indicate, to be comely, a portrait of the attractive state of the profession it served. To represent this goal, the founders placed on the front cover a picture of a hand holding up an empty mirror, devoid of any image, to the viewer/reader's gaze. As icon of both the journal and the studies it hoped to promote, *Speculum's* barren mirror thus invited the medievalist to cast his or her own image upon its vacant specular face. To do so, however, required a willed investment of the self (in effect, a narcissistic self-involvement) in order to generate those images by and through which to contemplate the meaning of the past.

In the choice of name and iconographic gloss, the founders of the Medieval Academy unconsciously underlined what was and remains the determinative

condition of possibility for the study of medieval history in America: absence. For like all countries formed by western European settlement since 1492, America lacks a medieval past. Any attempt to argue the importance and relevance of medieval history, therefore, must first overcome (or repress) its evident “otherness,” its utter alterity and lack of connection to any visible, shared national or cultural “American” past.

The “alterity” of the Middle Ages, of course, is hardly unique to the American consciousness of the era. Indeed, as Lee Patterson has repeatedly insisted, the Middle Ages has from the beginning served postmedieval Western historical consciousness as one of the primary sites of otherness by which it has constituted itself.² As constructed by Renaissance humanists, the Middle Ages comprised the West’s shadowy “other,” against which the Renaissance and modernity itself was defined, a modernity delineated above all by its difference from the premodern Middle Ages. As Patterson conveniently sums it up: “humanism, nationalism, the proliferation of competing value systems, the secure grasp of a historical consciousness, the idea of the individual, aesthetic production as an end in itself, the conception of the natural world as a site of colonial exploitation and scientific investigation, the secularization of politics and the idea of the state—all of these characteristics and many others are thought both to set the Renaissance apart from the Middle Ages and to align it definitively with the modern world.”³ From this perspective, the Middle Ages is precisely that, a millennium of middlelessness, a space of empty waiting and virtual death until the reawakening of the West to its proper nature and purpose in the period of the Renaissance.

For Europeans, the Middle Ages, if not modern, is at least “there,” evident in the monuments erected during those years and the traditions that stand presumptively at the origin of the modern European national states. It is, in fact, one of the peculiarities of medieval study everywhere that it constantly hovers between the dual consciousness of the Middle Ages as a place and time of nonorigin (that is, the dark, deathly period constructed in and by the Renaissance) and that of origin (the origin of the modern state). Caught in this double bind of nonorigin and origin, lack and plenitude, the Middle Ages, Kathleen Biddick has argued, can be “everywhere, both medieval and modern, and nowhere, sublime and redemptive.”⁴ It is, in part, this alterity—this “otherness”—of the Middle Ages that has given medievalists their sense of professional legitimacy, since the very strangeness and “difference” signified by the distant past suggests a special virtue required for its study. In America, however, the paradox of presence and absence common to medieval studies generally is incommensurably more acute; and precisely to the degree that the Middle Ages constituted an “absent other” in America, just so did the first American scholars insist, in a highly overdetermined fashion, on its place in a continuous stream of history stretching from the Teutonic past to the American present.⁵ To overcome absence and otherness, the original students of the medieval past in America construed alterity rather as identity. Given this, it is hardly surprising that the study of medieval history in the United States has from the beginning been marked by inherent paradoxes.

To begin with, although medieval civilization represented the triumphal past of "Catholicism" and "Gothic culture," a world organized according to the dictates of a deeply traditionalistic outlook on life and social customs, in North America its first historians tended to be Protestant, enlightened, and revolutionary founders. Thomas Jefferson and other early American revolutionaries were immersed in myths of Anglo-Saxon democracy, whose laws and chronicles, they believed, foreshadowed their ambitions for democracy. So indebted did Jefferson feel to Anglo-Saxon culture and what he took to be its legacy of Germanic liberties that he planned to put two Anglo-Saxon heroes, Hengist and Horsa—invited by Vortigern into Britain, according to Bede's *History of the English Church and People*, to aid in the defense of the country against enemies to the north—on the great seal of the new republic, whose obverse side would bear an image of the pillar of fire that led the Chosen People into the Promised Land (Exodus 13:21–22). According to John Adams, to whom he had communicated his wishes, Jefferson saw Hengist and Horsa as representing "the form of government we have assumed,"⁶ thereby tracing American democratic institutions to their origins in the social practices of the pre-Christian Germanic peoples.⁷ Jefferson cannot have read his Bede very carefully, though, since the latter made it clear that, although Hengist and Horsa had arrived in the guise of England's protectors, "nevertheless, their real intention was to subdue it," which, having done, Hengist become the founder of a royal line.⁸ More striking still was the coupling with the Old Testament pillar of fire, signifying not guidance or protection but an emblem of conquest, a vivid illustration of the young country's territorial ambitions.⁹ The underlying contradictions that marked such use of medieval figurations of American destinies would remain a characteristic feature of the American search for identity and origins in an absent and displaced medieval past.

To be sure, Henry Adams ushered in a new era by embracing with emotional intensity what were in some sense—at least from the perspective of Enlightenment thinkers—medieval history's most offensive aspects, but his passionate, slightly irrational celebration of the medieval past was not to be incorporated even into his own teaching of medieval history at Harvard. Adams illustrates a split in the approach to medievalism that was to continue for some time. In his writing he used the Middle Ages and what he saw as its vital, collective, organic culture as an exemplary counterpoint to the "anomic, dehumanized industrializing world that he himself inhabited."¹⁰ His *Mont-Saint-Michel and Chartres*, a work F. N. Robinson characterized as "that sensitive, poetic tribute of a skeptically minded, sometimes disillusioned modern to the spirit of the Middle Ages,"¹¹ turned to the Middle Ages as warrant for medievalism's antimodern agenda. In this, Adams participated in a burgeoning Romantic idealization of the Middle Ages that in America was largely the preserve of Catholic apologists who, like Adams, sought in the medieval world an idealized vision of an alternative social model against which the defects of the modern world could be judged.¹² In a famous chapter of *The Education*, Adams contrasted the spirit of the Virgin, to whom so much of the artistic and intellectual products of the High Middle Ages were dedicated, to that of the modern dynamo, image of the materialistic, de-

humanizing greed and technology of the modern age.¹³ For Adams, what was attractive about the Middle Ages was precisely its alterity; it was, he observed, "the most foreign of worlds to the American soul."¹⁴ The New World, Adams believed, had not inherited medieval institutions, patterns of social organization, or religious beliefs; the study of medieval history, therefore, could offer no great truths or lessons for the guidance of American life.¹⁵ Its utility, by implication, lay merely in the escape that it provided from the increasingly harsh realities of the modern world, a realm of fantasized otherness in which to locate the antimodernist self.

Thus, in 1871, when Harvard president Charles W. Eliot invited the young Adams to teach the Middle Ages at Harvard, Adams could think of little to offer his students but the dry facts of political and legal history, learned during his two years of advanced historical training in Germany, where he had been taught to read documents in the new, philologically oriented, manner of the German seminar. For seven years, as lecturer in History 2 (forerunner of the modern Western Civilization course), Adams taught the stuff of history with all the discipline and purposeless of antiquarian research.¹⁶ Adams's legacy to the study of medieval history in America was thus a double and divided one: his writings articulated a conservative strain in American medievalism which would serve as a refuge for those wishing to retreat into a world of preclass, preindustrial society.¹⁷ His teaching, on the other hand, inaugurated what was to become, under the leadership of Charles Homer Haskins, an almost exclusive concern with the political and institutional development of the monarchical states of northern Europe, in particular England and France, that persisted virtually down to the present time.

If Adams was the first to teach medieval history professionally in America, Haskins was America's first true professional medieval historian.¹⁸ Moreover, if Adams represents American medievalism's antimodernist agenda, Haskins was the first and most powerful figure in promoting its modernist agenda. And like his Enlightenment forebears, to whom as a progressive Democrat he was heir, Haskins was to do this by resolutely stressing the continuity of the American present with past medieval institutions.

Haskins came from an affluent Protestant family in Pennsylvania. A child prodigy, he learned Latin and Greek from his father before he was seven, and at the age of fifteen he entered a local college, from which he transferred to Johns Hopkins in his second year, graduating from Hopkins with a B.A. in 1887 and a Ph.D. in American history from Herbert Baxter Adams (in 1890) by the time he was twenty. From Hopkins, Haskins went to Wisconsin to teach American history but after a few years determined to become a medievalist, and so, as required for aspiring medievalists at the turn of the century, he decamped for Europe, entering France's prestigious *Ecole des Chartes*, designed to train the country's archivists and (in that period) historians in the scientific investigation of medieval documents that goes by the name of *diplomatics*. After a half dozen years spent in study at the *Ecole des Chartes* and travel to various archives in England, France, and Sicily, Haskins accepted a professorship at Harvard in 1910. At Harvard he subsequently became dean of the Graduate School of Arts and Sciences,

which delayed his major publications until the last half of his academic career, roughly the period from 1918 to 1929. In 1928, three years before the stroke that would incapacitate him, he found his successor in Joseph Reese Strayer, a graduate of Princeton who came to study with him at Harvard before returning to Princeton to teach for the remainder of his career. Between them, Haskins and Strayer were to direct and dominate the practice of medieval history in North America from the 1920s down through the late eighties.¹⁹

Haskins's formation at Hopkins was to have an enduring impact on his career and ideas. The Department of History had graduated Woodrow Wilson but a few years earlier, and throughout his life Haskins would prove an ardent Wilsonian progressive (i.e. Democratic liberal), sharing with Wilson a deep faith in progress, rational reform, and the benefits of government, beliefs that significantly shaped his historical practice. Not content to implement his views in the classroom, Haskins accompanied Wilson to the Paris Peace Conference in 1919 and 1920 as one of three principal advisers. With Robert Lord, he helped to create Czechoslovakia and Yugoslavia, states carved out from the Austro-Hungarian Empire.²⁰ On returning to Cambridge, he assumed the directorship of the American Council of Learned Societies, from which position he helped to found and finance the Medieval Academy of America in 1925 and its new journal, *Speculum*,²¹ both intended to signal the coming of age of American medieval studies by rivaling in seriousness, exacting standards of scholarship, and formal (not to say deliberate) dullness the great academies of European learning, on which these American institutions were consciously modeled.

Medievalism's modernist agenda that Haskins sought to implant on American soil in its broadest sense took the form of an alliance between positivism, Idealism, naturalism, and objectivity, many of whose components derived, ultimately, from the German scientific historiography of the late nineteenth century, but which Haskins was to cast in a distinctly American, early-twentieth-century, progressivist mold.²² To do so, however, Haskins had first to cover the absence of a medieval past in America, to guarantee the relevance of medievalism to precisely the vision of continuity and progress that informed his activities both as a professional historian and as an adviser to President Wilson. Few American historians have argued the relevance of medieval history to Americans as eloquently or with as profound conviction as Haskins. While recognizing, as he said, that "American history is our first business," it was not, he believed, "our sole business," and in any case, the two were ultimately part of the same story. European history, Haskins argued in a 1923 essay, "European History and American Scholarship," published in the *American Historical Review*, is "of profound importance to Americans. We may at times appear more mindful of Europe's material indebtedness to us than of our spiritual indebtedness to Europe; we may in our pharisaic moods express thanks that we are not even as these sinners of another hemisphere; but such moments cannot set us loose from the world's history. Whether we look at Europe genetically as the course of our civilization, or pragmatically as a large part of the world in which we live, we cannot ignore the vital connections be-

tween Europe and America, their histories ultimately but one.”²³ And of all the available European pasts, Haskins signaled America’s natural affinity with that of England, for, he declared, “English history is in a sense early American history.”²⁴

This insistence on continuity and relevance was institutionalized subsequently in the founding of the Medieval Academy and *Speculum* in 1925, whose embracing purpose was to promote American study of the Middle Ages in all its varieties and subdisciplines in order to help Americans, wrote George R. Coffman in the official report of the foundation, “to comprehend our medieval ancestors.” Help was needed, he confessed, given the obscure and complex nature of medieval civilization, and it would require the “cooperation and the creative energy of students of art, archeology, folk-lore, government, law, literature, medicine, philosophy, theology and all other branches” of knowledge to elucidate.²⁵ Thus, from its inception, the professional study of the Middle Ages in America disclosed a durable structure of paradox in American medievalism—the sense of the absolute remove of the medieval past, its strange, difficult, occult nature, combined with an equally absolute sense of filiation with it.

Haskins was not unaware of this paradox and in his books and essays sought to resolve it in directions that would promote the modernist agenda for which his appropriation of the medieval past stood. His enduring tribute to the modernity of the medieval past was his work *The Renaissance of the Twelfth Century*, published in 1927, in which he contested the master narrative of Western civilization according to which the modern world began in the Renaissance. But, Haskins insisted,

the continuity of history rejects such sharp and violent contrasts between successive periods, and . . . modern research shows us the Middle Ages less dark and less static, the Renaissance less bright and less sudden than was once supposed. The Middle Ages exhibit life and color and change, much eager search after knowledge and beauty, much creative accomplishment in art, in literature, in institutions. The Italian Renaissance was preceded by similar, if less wide-reaching movements; indeed it came out of the Middle Ages so gradually that historians are not agreed when it began, and so would go so far as to abolish the name, and perhaps even the fact, of a renaissance in the Quattrocento.²⁶

Thus, instead of viewing the Middle Ages as Western civilization’s premodernity, Haskins pushed the beginnings of modernism back to the twelfth century, thereby strengthening at one and the same time the continuity of the Middle Ages with the present and the centrality of its study as the seedbed or parent civilization of the modern West. Although little read today except for its genuine contributions to the history of science,²⁷ Haskins’s argument in *The Renaissance of the Twelfth Century* for the modernity of the Middle Ages began that “revolt of the medievalists”²⁸ which sought a new legitimacy for the medievalist’s professional identity against the charges of obscurantism, irrelevance, and technical virtuosity that continually haunted the practice of medievalism in America in the face of its clear lack of connection with national identity.

Making a virtue out of necessity, Haskins argued that America's lack of direct connection with the Middle Ages cultivated detachment on the part of its scholars—"one of America's great advantages as regards many aspects of European history . . . enabling the historian to trace [the history of European civilization] without those national prejudices from which his European confreres cannot wholly emancipate themselves,"²⁹ he claimed—thus reinforcing the scientific character of scholarship done in the German, positivist mold. In that sense, the very alterity of the Middle Ages abetted the entrenchment of positivism as *the* scientific form of scholarly method in American medieval historiography, whose counterpart in literary study was an equally fervent espousal of philology, both part and parcel of the specific kind of "source criticism" or *Quellengeschichte* that American scholarship generally learned during its early period of tutelage in the German seminar.³⁰

Translated into the realm of historical practice, Haskins's positivist objectivity and German-style historicism took the form of a search for the rational basis of the political and administrative development of monarchical institutions in Europe, especially those of the Anglo-Normans and French. Like Wilson an admirer of the British constitution and political achievement, Haskins focused his attention on the Normans, whose governmental genius he believed had reconstituted the British political system after the Norman conquest of 1066, bringing to the disordered and backward Anglo-Saxon realm the peculiarly systematized and centralized form of feudalism that the Normans had first developed in France. The fruits of this research had begun to appear in articles after Haskins started teaching at Harvard, but his magisterial work *Norman Institutions* was not published until 1918, thus favoring a more widely based reorientation in American medievalism away from the study of German/Anglo-Saxon history after World War I.³¹ Hence one effect of Haskins's concentration on Norman institutions was to maintain the traditional orientation of American scholars toward British history but at the same time subtly to redefine what was best in Britain as "French" (or Anglo-Norman), thus permitting American scholarship to evade any possible stigma attached to German history as a result of the war, a move more than validated (and strongly reinforced) by the outcome of World War II.³²

In Haskins, the influence of Wilsonianism can be seen in his focus on the inherent rationality of the Norman brand of feudal organization, with its tendency to centralize, hence place power in the hands of a court elite, at the expense of an anarchic baronage, and its establishment of political and judicial order to bring peace and stability to the realms under Norman sway, in Sicily as well as England.³³ The lesson that medieval monarchies thus bequeathed to the American present was the power of government to effect unity and consensus out of fragmentation and discord. And no one was to sound this lesson more clearly than Haskins's premier student, Joseph Reese Strayer.

Strayer shared with his mentor a dedication to the investigation of what he called "the medieval origins of the modern state,"³⁴ in particular by studying the growth of royal bureaucracies, governmental powers, and the legal and constitutional principles by which medieval kings were able to secure not only the ability

to rule through force but also the affection and loyalty of their subjects. As in the case of Haskins, the focus on monarchy was more or less accidental, and Strayer's real concern was for the elements that promoted governmental stability and effectiveness and allowed the state to protect its subjects.³⁵

Strayer's Harvard dissertation for Haskins, published as *The Administration of Normandy under Saint Louis*,³⁶ continued his mentor's focus on the Normans, but in a Normandy reintegrated into the French realm as a result of its reconquest by Louis's grandfather, Philip Augustus, in 1204. Once again, the questions Strayer posed were Haskins's questions concerning the impact of a specifically Norman style of government, now upon the French monarchy. In particular, he wondered if Norman customary law had tempered the activities of the Roman lawyers of the French crown in the thirteenth century, while teaching them how to develop their own systems of administration and taxation.³⁷ Behind this question stood the desire to reinterpret French monarchical institutions in such a way as to make them compatible with American democratic principles, to divest the French monarchy (at least in the Middle Ages), that is, of the charge of absolutism, a form of political governance that Strayer found personally distasteful and historically irrational and ineffective, since he fervently believed that despotic regimes were naturally weak by virtue of their inability to win their subjects' adherence.

Strayer's attempt, in effect, to "Americanize" royal history in the Middle Ages proceeded along three lines. The first, which owed most to Haskins's influence, was to argue for the innovative, ameliorative impact of the centralizing monarchies in twelfth- and thirteenth-century England and France, whose actions brought order out of chaos and national unity out of feudal fragmentation. Government, as such, was a "good" thing, securing for its subjects the necessary peace and stability that enabled them to prosper. Moreover, and most important, medieval kings like Henry II of England and Philip the Fair of France achieved these results not through violence but by instituting a legal system able to deliver cheaper and more effective forms of justice to their subjects. Royal centralization, therefore, far from tending to absolutism, was the first step in the implementation of Western constitutionalism, a rational system for the adjudication of national issues and a style of government beneficial to subjects of the king. Strayer devoted a lifetime to demonstrating that this, *not absolutism*, represented the true achievement of medieval monarchies. The result of this work was his famous article, "Philip the Fair—a 'Constitutional King,'" published in the *American Historical Review* in 1956, in which Strayer argued, against the grain of previous scholarship, that Philip the Fair, far from representing a capricious, tyrannical king who used a rising class of lawyers brandishing the principles of Roman law to argue for the status of the king as beyond the reach of law (*rex legibus solutus est*), was instead a "constitutional" king, who used legal principles to ensure the welfare and security of his realm to the benefit of his subjects.³⁸ After being criticized for this view by scholars,³⁹ Strayer later, in his monumental work, *The Reign of Philip the Fair* (1980), modulated his position to emphasize instead the efficiency and efficacy of Philip's government, in lieu of the somewhat

anachronistic claims concerning royal “constitutionalism” in the 1956 article, but his underlying point remained the same: strong and legitimate government was a positive force in society and in the history of western European state-building.

Strayer was aware, of course, that in France the monarchy ultimately took an absolutist turn, for which he offered a basically “geographical” explanation. In a series of interesting essays,⁴⁰ Strayer argued that the reason that England became a true constitutional monarchy, with effective parliamentary government, was due to its restricted size and early centralization. Because the realm was small and highly organized by English monarchs, who drew upon their subjects’ services in the administration of law, it fostered unity among the barons, who, when the monarchy turned capricious under King John, were able to band together to oppose royal power and, ultimately, to institutionalize that opposition in the creation of Parliament. France, in contrast, was too large and too late in developing habits of centralized consultation for this to occur. Because the king was so long weak, the barons had little motive to unite against him, and once the monarchy became powerful, as it did beginning with Philip the Fair, it was too late for the barons to develop those habits of cooperation and concerted action that in England combined to produce a parliamentary form of government. Instead, French kings, when they needed to consult their subjects over questions of taxation, tended to do so by individual region rather than in a unified assembly, promoting fragmentation and particularization among the nobility, which worked ultimately to the monarchy’s advantage. For this reason, the Estates-General in France never developed in the same way as Parliament in England, and France took an absolutist turn that would, to be sure, call forth its corresponding opposition in the French Revolution (thus confirming Strayer’s deeply held belief that absolutist regimes never finally succeed). The effect of this “geographical” argument was to exculpate the king of any charge of tyranny, since it represented a historical constraint that medieval monarchs simply did not have the resources to overcome. The “moral” upshot of this argument was to preserve the “virtue” of the French king as a legitimate and lawful ruler, who held true to the principles of rational, just government, even if in the end he was betrayed perforce by the recalcitrant conditions of the realm he governed.

The “virtuous” character of medieval monarchy—an analogue, no doubt, of the American virtue that both Haskins and Strayer sought to extol and promote—can best be seen in the second of Strayer’s main lines of research, a series of articles dedicated to demonstrating the ideological means by which royal government in the Middle Ages was able to procure and maintain the loyalty and affection of the governed, affirming along the way Strayer’s conviction that no government could rule by violence alone. In articles like “Defense of the Realm and Royal Power in France,” and “France: The Holy Land, the Chosen People and the Most Christian King,”⁴¹ Strayer argued powerfully that French kings had succeeded in winning the devotion of their subjects by successfully articulating the legitimate basis of their rule and, especially under Saint Louis, by presenting themselves as rulers worthy of affection and obedience, producing a cult of kingship in France that was centered on the person of the ruler. It was this ideological

legitimacy, a mystique of monarchy that encouraged “Frenchmen” to look to the king as the focal point of an emergent sense of national identity, and not the deployment of powers of coercion, that fundamentally explained the success of the French medieval monarchy. So effective were the administrative systems put in place by medieval governments, and so secure the loyalty of their subjects, that the emerging national states of Europe, Strayer argued in his presidential address to the American Historical Association in 1971,⁴² were able to withstand the crises of the fourteenth century, in sharp contrast to the Roman Empire, doomed to succumb to the vagaries of the fourth century precisely because it lacked the bureaucratic mechanisms and affective legitimacy that medieval kings had successfully brought into being.

The precondition for these developments, and the third vector of Strayer’s research, was what he termed the “laicization” of society in the thirteenth century.⁴³ By this term Strayer meant something close to Weber’s “disenchantment” of the world, a tendency to place faith in human rather than divine figures, and the human figures who became the repository of that faith were, of course, kings. In claiming that the thirteenth-century medieval world was increasingly secular in outlook and sentiment, Strayer challenged the core image of that “greatest of centuries” (the title of Catholic historian James J. Walsh’s book on the period) and the conviction that what made medieval monarchs powerful were the sacral (not the judicial) powers that they exercised.

Beginning in the 1930s and continuing on until the 1980s, Strayer’s long career of teaching and research on the writing of medieval history characterizes a dominant (though by no means exclusive) orientation of American scholars of his era, a group that includes Charles McIlwain and Charles Taylor at Harvard, Sidney Packard at Smith College, Carl Stephenson and Brian Tierney at Cornell, and Sidney Painter and John W. Baldwin at Johns Hopkins, Bryce Lyon at Brown University, Thomas Bisson at Berkeley and Harvard, as well as C. Warren Hollister at the University of California at Santa Barbara, Gavin Langmuir at Stanford, and Robert Brentano at Berkeley, to name only a handful, all of whom were centrally concerned with questions of legal/constitutional and institutional history in relation to issues of both feudalism and state formation. Over the course of half a century these men trained generations of students whom they sent out throughout the country, populating centers of medieval study from the East to the West Coast. If one includes Haskins, between them they span virtually the entire length of professional medievalism in America, shaping it with their notions of scientific methodology, rationality, and progressive ideology. A parallel consequence of their dominance, Norman Cantor has argued, was to leave medieval studies in America firmly in the hands of “a small, enclosed world of determined, middle-class WASPs, ruling unchallenged (before the German-Jewish emigration of the late thirties) on the history of Roman Catholic Europe.”⁴⁴ And it was precisely a sea change in the recruitment of medievalists in the sixties and seventies that was to change the face of American medievalism almost beyond the point of recognition. This new generation, entering graduate school in the sixties and the profession in the midseventies, completely reoriented the study of

medieval history in America, creating a new landscape of concerns that could hardly have been anticipated. In this, medieval history was scarcely alone. The changes it experienced were part of a much broader movement which, from the perspective of the nineties, can be seen as the importation and adaptation of postmodernism⁴⁵ into the heart of American scholarship in all fields.

In some ways, medieval studies might have been thought to be ideally placed to exploit the historicist strain in postmodern thought, since it had always insisted on *difference* ("alterity") as the privileged category defining the relationship of the Middle Ages to the modern world of scholarship. Given that a dominant impulse in postmodern criticism is precisely the attempt to "think" difference, that is, as Eric Santner explains it, "to integrate an awareness of multiple forms of otherness, to identify . . . across a wide range of unstable and heterogeneous regionalisms, local knowledges and practices,"⁴⁶ medievalists were in principle predisposed to the hermeneutic posture that postmodernism demanded of its practitioners. Moreover, the vaunted complexity of medieval documents, the necessity for highly technical approaches to them, implied that meaning in medieval texts was *not* naturally accessible and that such texts were, by nature, opaque, at least to the modern reader. In that sense, philology—the principal technical apparatus in the medievalist's arsenal of interpretation—might have seemed compatible with the emerging sense of the opacity of all writing (of writing as *différance*, in Derrida's sense) and with the turn to textuality as the matrix and condition of possibility for all forms of knowledge. Similarly, the sense of marginality, and the quest for it, that haunts the postmodern should be equally congenial to the medievalist, whose object of study lies outside the master narrative of Western modernity and whose own relationship to the profession is often considered to be, if not marginal itself, at least of marginal utility in a national environment committed to innovation and relevance.

And yet, American medievalists—and among them, historians in particular—have been slower than almost any group in the academy to take up the challenge of postmodernism. In part this was due to the highly overdetermined nature of the discourse of continuity and progress that had marked the American relation to its patently absent past virtually from the time of Jefferson on, and which had subtended the modernist agenda of the profession in its very formation. In part, and somewhat paradoxically, it was also due to the conservatism of some who joined the profession, for whom the Middle Ages retained its appeal as an alternative model of social being, belief, and intellectual elitism. (Medieval history was, after all, hard to do, demanding a mastery of languages that few Americans naturally commanded.) And in part, it may also be due to the sensed implication that the very disarray of modernism that *postmodernism* by definition portends threatens to deprive the Middle Ages of whatever *negative* interest it once had as the refuge of the unenlightened, irrational, and "other."⁴⁷ In all these ways, the arrival of postmodernism must have seemed to undermine the unstated but nonetheless powerful investments of the self that medievalists brought to their work and in which they mirrored their professional identities. It was, therefore, not until the seventies, at the earliest, that there began to appear those currents

of thought in medieval historical scholarship in America that can be linked to the influence of postmodernism.

In my view,⁴⁸ there were three dominant trends in historical work in the late seventies and eighties that made themselves felt in the domain of medieval historiography and that, in sum, constituted a virtual “revolution” in the American (and, in the Haskins-Strayer sense, “Americanizing”) writing of history. The first constituted a rejection of the positivist certainties and foundationalism of the “old” historicism—together with its implicit, universalizing humanism—in favor of a “new” historicism that took its lead from the creation of “discourse” studies written under the sign of Foucault (at least initially) and which resulted in a social “constructionist” approach to the past that would issue, ultimately, in the practice of “cultural history.”⁴⁹ Another way of characterizing this shift is as a transformation in the idea of history from a narration of, in the old Rankean formulation, *wie est eigentlich gewesen*, to history as representation, a recognition that the investigation of the past occurs only through the mediatory and mediating texts that it bequeaths and that, therefore, what is “recovered” is not so much the “truth” of the past as the images of itself that it produced, images conditioned, indeed determined, by its ambient, and historically determinate, discourses.

Second, and closely allied to this shift, was the so-called linguistic turn, or what might, in its most general sense, be termed a transformation in the understanding of documents as texts rather than sources. For medievalists, this shift, conducted under the impact of both symbolic anthropology of the Geertzian sort and semiotics (and, in part, Derridean deconstruction, though Derrida’s influence was felt primarily in the field of criticism, rather than history), contested the positivist and philological center of all medieval studies, and is perceived by the older generation of medievalists in America as a threat to the very enterprise of medievalism in America. This because, in treating documents as texts rather than sources, it suggests the instability and opacity of all and any knowledge of the past, while at the same time (perhaps more importantly?) attacking the very foundations on which medievalists had constructed their professional legitimacy, involved as it had always been with mastery of highly technical (rather opaque) fields such as paleography, diplomatics, codicology, etc., not to mention all those “dead” languages. Together, these two movements are creating a “new medievalism” (in the title of a recent collection of essays) that is, in Eugene Vance’s words, “a science not of things and deeds but of discourses; an art not of facts but of encodings of facts.”⁵⁰

The third (chronologically earliest) transformation came about as a result of the emergence of American feminist historiography and, ultimately, gender studies, whose impact was to shift attention away from precisely the “public” sphere that had engaged the work of American medievalists in the Haskins-Strayer tradition to the private, domestic, and, increasingly, carnal (that is, bodily) spheres. Although initially feminist historiography concerned itself with demonstrating the presence of women in the Middle Ages, making them “visible” as actors upon the historical (if not public) stage—a strategy of *inclusion*, of reading women into the then dominant historical discourse—it quickly developed into a much

broader interrogation of the very basis of a practice that claimed “truth” while omitting from its purview fully half the population, a result of which was to demonstrate the ways in which patriarchy itself (especially in its highly misogynist, medieval variant) relied upon a gendered view of nature and power for its success.⁵¹ From there it was but a short step to an exclusive concern with women themselves, a concern that has been especially prominent in the field of medieval spirituality⁵² and literary study,⁵³ where the search for authentic women’s “voices” is producing highly paradoxical uses of poststructuralist interpretations of the extant texts.

While these changes have characterized American historiography in general from the seventies on,⁵⁴ in the field of medieval history, what might, for the sake of symmetry, be here called medievalism’s postmodernist agenda required a prior, and double, analytical move: first, a “demodernization” of medievalism’s modernist project that had stood at the core of virtually all medieval disciplines since the late nineteenth century and had endowed American medievalists especially with a professional purpose and identity; second, a (postmodern) “defamiliarization” of the resulting—demodernized—cultural artifacts, an analytical gesture that at the moment appears to entail a certain “demonizing” of the Middle Ages, the corollary of which is what Paul Freedman has called “the return of the grotesque in medieval historiography.”⁵⁵ What is taking place, therefore, is not so much the product of the unearthing of new texts (although, inevitably, it has led to the discovery of them) as a massive interpretive shift in the meaning of the Middle Ages that has emerged as a consequence of a complete refocusing away from the normal to the contested, from an optimistic and “progressive” decoding of the past to a reappropriation of its otherness,⁵⁶ an alterity now construed not merely as the boundary demarcating the premodern from the modern but as a radical form of “otherness” that almost defies comprehension.

The three directions of change I have indicated might all be seen as aligning themselves beneath the Foucauldian banner: *inquiéter tous les positivismes*—to disrupt all forms of positivism. I am not trying to suggest that Foucault has been the determinative influence on the development of American medievalism’s postmodern agenda. Indeed, if one takes literary studies and their impact on historians into account, equal, if not greater, weight must be given to semiotics and deconstruction. But since Foucault committed himself to working through the implications of postmodernism within history, it has to some extent been easiest for medieval historians to absorb the principles of postmodernism via his writings.

Foucault’s work has been especially influential within the domain of historicism, where he has argued that, in a postmodern age, the problem of history “is no longer one of tradition, of tracing a line, but one of division, of limits; it is no longer one of lasting foundations, but of transformations that serve as new foundations”;⁵⁷ which is to say that history is a form of archaeology. To take Foucault’s notion of archaeology seriously, therefore, meant abandoning the master narrative of continuity and progress that had informed historical practice at least since the nineteenth century (indeed, earlier) in favor of a fractured, discontinuous,

and ruptured sense of the past. As a practical matter, it has promoted a concentration on small microhistories which are no longer assumed to reside within a larger, lineal network of continuous relationships. If genealogy once meant for historians the tracing of direct lines of descent from past to present, under the sign of Foucault it now stands for all that is contingent, invasive, aleatory in history, the constant irruptions and disruptions, misalliances and failures that mark familial relations over time.

At the same time, the implications of a Foucauldian notion of discourse make a belief in objectivity, positivism's ethical twin, virtually untenable, since no thought (or thinker) can escape the knowledge-power systems of its own historical, archaeologically disjunct, era, thus problematizing in fundamental ways the transactions between past and present required for genuine historical understanding, creating a seemingly unbridgeable hermeneutic gap. Within the domain of textuality, Foucault's archaeological metaphor points to the treatment of documents as "monuments," that is, as "mute," as that which no longer "speak" to us clearly and directly from the past but must be submitted to an intrinsic analysis (like the silent stones of the archaeological site) before they can be made to yield up their secrets.⁵⁸

Within women's history, Foucault's constructivist view of discourse, when applied to issues of sexuality, has powerfully abetted the feminist view that sexual categories that were once thought to be natural, universal, and given, the very bedrock of identity and being, are instead historically produced under determinate, discursive conditions and in the service of specific material (patriarchal) interests and power relations. Thus gender differences have themselves been revealed as part of a master narrative that, in unmasking, feminist historiography seeks to dethrone. While few medievalists have followed feminists like Judith Butler in affirming a wholly performative notion of gender, the very instability, lability, and obscurity of medieval notions of sexuality have lent themselves readily to this kind of treatment.⁵⁹ In particular, Caroline Bynum's work on late medieval spirituality has disclosed the centrality of the body and bodily practices to a form of asceticism that is peculiarly female, both in its recourse to food as a central symbol of transcendence (in particular, through consumption of the Eucharist) and in its highly penitential, self-punishing mode of bodily deprivation (fasting, self-flagellation, etc.).⁶⁰

And finally, Foucault's attack on the normalizing mechanisms of modern epistemological regimes has promoted a sensitivity to ways in which knowledge-power systems marginalize and exclude—silence, in effect—some while valorizing others, and has led medieval historians to take a fresh look at the operations of the church and its systematic theology in the High Middle Ages as well as to seek out those elements of medieval society that both contest and thus seem to escape their power. The result of this view of the "normalizing" tendencies of all discursive formations and the desire to undermine their efficacy has been, within medieval history, a complete reinterpretation of the thirteenth century as witness to what has been called "the rise of a persecuting society."⁶¹ Thus, the "greatest of centuries" is no longer seen as the center of a modern, rational, progressive

movement but as a Foucauldian panopticon of discipline and colonization, seeking out in order to tame and punish all those perceived as dissenting from the church's regime. This has encouraged, as its obverse, new interest in heretical groups,⁶² in Jews and in Jewish-Christian relations,⁶³ in children, in popular culture, in gays and other marginalized groups.⁶⁴ Subjects once themselves marginalized are inching toward the center of concern: Pope Joan, the inquisition, visionary hysteria, the *droit de seigneur*, and the like. If the latest meetings of the Medieval Academy are to be trusted, multiculturalism, postcolonialism, "orientalism," and "sodometries" are soon to follow.

What is particularly striking about medieval work done in this vein, moreover, is the degree to which it focuses not only on the marginal but on the grotesque. Thus Bynum trains her eye on extraordinary acts of asceticism among the women she treats, who drank seeping pus from wounds, fasted to the point of starvation, and submitted to horrifying acts of self-deprivation all in the name of spiritual transcendence. Jewish historians have recently returned to the study of the massacres of 1096, with their images of piles of dead and mutilated bodies.⁶⁵ Even within the most traditional domain of feudal studies, there is a growing emphasis on violence as the engine that drives the feudal machine.⁶⁶ Indeed, the latest work on the Normans, Eleanor Searle's *Predatory Kinship and the Creation of Norman Power*, stresses the violent, ritualized nature of their exercise of power, in sharp contrast to Haskins's view of the rational, systematic nature of Norman feudalism. Thus violence, conflict, and marginality are producing similar effects in many fields of research: the "defamiliarizing of what previously seemed canonical, progressive, and modern in favor of the ironic⁶⁷ and fantastic."⁶⁸

If one inquires into the reasons for the emergence of these new currents in the practice of medieval history in America, the answer, it seems to me, lies not so much in the impact of postmodernism per se but in the reasons for the American receptiveness to postmodernism's agenda. And to understand these reasons it is necessary to return to the social recruitment of American medievalists in the sixties and seventies. In addition to the entrance of women and blacks into the American academy for the first time, there was also a new wave of participation among classes and what, for lack of a better word, can be called ethnic groups, among them Jews, all of whom entered the university in newly massive numbers in the early sixties, thus constituting a clientele whose interests needed to be addressed and a pool from among which future professionals could, and would, be recruited. Hence, John Van Engen, in seeking to understand the motivations that have prompted Americans to take up the study of the Middle Ages, in whatever aspect, in light of its absolute remove in space as well as time from their personal and/or familial experience, has pointed to the ambivalence with which these "new" groups of Americans have approached the study of European, and specifically medieval, history. Even for those with cultural roots in Europe, Van Engen believes, most came from among peasants, the unfree, or dispossessed, retaining, therefore, "little personal stake in the old European order." Moreover, Van Engen insists, "the sting of that removal was real . . . the heirs to those immigrants have never been able to decide whether they should spitefully keep their

distance, avoiding the old corruption, or return to Europe with pent-up intensity, reclaiming or making space for all that was once denied them. The study of the European Middle Ages remains for Americans a continuing dialectic between connection and disjunction, the tug of social and cultural features still influential among us and the shimmer of something totally and yet perceptibly other."⁶⁹ Surely this, together with the influence on American scholarship of the German-Jewish refugees and their children, provides one of the profound reasons for the current disorientation, or to put it more positively, reorientation, in the study of the Middle Ages. For ours is the first generation of those immigrants, both from among the dispossessed of the "old European order" and the refugees from Hitler's Europe, to enter the American academy in large numbers, bringing with it all the ambivalence toward and desire for mastery over that world we have all, in some deep way, lost.

Given this, it is hardly surprising that the most powerful sense of the Middle Ages current in the academy is what goes under the name of its "alterity," for that hermeneutic alterity offers the best means of escaping from the model of total (and totalitarian) identification which was the chief mode of studying the Middle Ages in the past. In that sense, as Robert Stein recently suggested to me, "in its resistance to totalitarian identifications, the position of loss may well be an advantageous position from which genuine scholarship can proceed." Alterity, from this perspective, is the name we give to the recognition that the past inevitably escapes us, that words, names, signs, functions—our fragile instruments of research and scholarship—are at best only momentarily empowered to capture the reality of the past, the knowledge of which as a lived, experienced, understood repository of life is always slipping away, if indeed it was ever knowable to begin with.

What has changed in the postmodern understanding of medieval alterity, and serves sharply to distinguish it from the earlier modern construction of it, is the simultaneity of our desire for history and the recognition of its irreparable loss, a recognition that paradoxically nourishes the very desire it can never satisfy. This desire has, therefore, an elegiac component, in which it is transformed into a kind of mourning for the unpossessed (or lost) "other." In postmodern historiography, I would argue, the tension between our sense of the past's erasure through the annihilation of memory and our desire for history harbors a longing for presence, a presence we simultaneously acknowledge as always already absent, and thus like the past itself, an unattainable object of desire. Thus what I call the desire for history not only represents the desire to recuperate the past or the other but also marks the inaccessibility of that absent other, an irony that seems to me to be the very figure of history in the late twentieth century.

Our desire for the past is, thus, borne alongside our recognition of its loss, a loss we no longer can, or care, to mask beneath the modernist guise of continuity and progress. If postmodernism has seemed to this generation a viable, indeed crucial, theoretical context out of which to work, this is so, I believe, because postmodernism invites us to mourn, as Eric Santner has written, "the shattered fantasy of the (always already) lost organic society that has haunted the Western

imagination."⁷⁰ The "alterity" of the Middle Ages, it would appear, is our own estrangement from that fantasy writ large. On the cover of *Speculum*, there is no longer a mirror.

NOTES

1. The author would like to thank the members of the San Marino conference and Professor John W. Baldwin for their helpful criticism of this paper as well as to acknowledge a special debt to Dorothy Ross, who has been consistently willing to instruct a neophyte in American history and the development of social science methodology in the subtleties of the subject. I never fail to learn from her, whether through reading or conversation. "Editor's Preface," *Speculum* 1 (1926): 4.

2. Lee Patterson, introduction, "Critical Historicism and Medieval Studies," in Lee Patterson, ed., *Literary Practice and Social Change in Britain, 1380–1530* (Berkeley, 1990), p. 2. See also his *Negotiating the Past: The Historical Understanding of Medieval Literature* (Madison, 1987) and "On the Margin: Postmodernism, Ironic History and Medieval Studies," *Speculum* 65 (1990): 87–108.

3. Patterson, "Critical Historicism and Medieval Studies," p. 2.

4. Kathleen Biddick, "Bede's Blush: Postcards from Bali, Bombay, Palo Alto," in John Van Engen, ed., *The Past and Future of Medieval Studies*, Notre Dame Conferences in Medieval Studies, IV (Notre Dame, 1994), p. 16.

5. On the "Teutonic germ" theory of institutional history, which claimed that the seed of American democracy had been created in the Black Forest, taken to Anglo-Saxon England, and thence across the ocean to America, see W. Stull Holt, "The Idea of Scientific History in America," *Journal of the History of Ideas* 1 (1940): 352–62; Dorothy Ross, "On the Misunderstanding of Ranke and the Origins of the Historical Profession in America," *Syracuse Scholar* 9 (1988): 31–41; idem, *The Origins of American Social Science* (Cambridge, Eng., 1992); idem, "Historical Consciousness in Nineteenth-Century America," *American Historical Review* 89 (1984): 909–28. A particularly egregious example of historical work done in this vein is Herbert Baxter Adams, "The Germanic Origins of New England Towns," *Johns Hopkins University Studies in Historical and Political Science* 1 (1883): 5–38. For the "rhapsodic racialism" that ran through the intellectual agenda of Baxter Adams's seminary at Johns Hopkins University see Marvin Gettleman, ed., *The Johns Hopkins University Seminary of History and Politics: The Records of an American Educational Institutions, 1877–1912*, 5 vols. (New York and London, 1987–90). Although especially dominant in the work of Baxter Adams and his students, the Teutonic germ theory was equally prevalent in many writings of medievalists around the turn of the century and is a central premise of the research and writing done in Henry Adams's seminar on Anglo-Saxon legal institutions during his seven years of teaching at Harvard. The work of the seminar was subsequently published as *Essays in Anglo-Saxon Law*, ed. Henry Adams (Boston, 1876), to which Adams himself contributed an article, "The Anglo-Saxon Courts of Law." Unlike Baxter Adams, however, Henry Adams's espousal of the theory was tepid at best, and not of long duration, though it does inform his work on the hundred courts that resulted from his Harvard research seminar.

6. Cited in Allen J. Frantzen, *Desire for Origins: New Language, Old English and Teaching the Tradition* (New Brunswick, 1990), p. 16. Another design formulated by a committee composed of Benjamin Franklin, John Adams, and Thomas Jefferson—also rejected—

derived from a drawing by Pierre-Eugène du Simitière (a Swiss painter living in Philadelphia) and consisted of a shield divided into six sections, on which the arms of England, Scotland, Ireland, France, Germany, Belgium, and Holland were painted. Above the shield would be the "Eye of Providence in a radiant Triangle"; below the motto *E Pluribus Unum*. As Jay Fliegelman indicates, "what is fascinating about the design is that in its first official appearance, the *E Pluribus Unum* motto refers as much to the process whereby America derived from the six 'countries from which these States have been peopled' as it does to America as a union of States." Hence the insistence on continuity with the European past, despite the more radical implications of the Revolution itself, was to be inscribed in America's emblematic self-representation. On this see Jay Fliegelman, *Declaring Independence: Jefferson, Natural Language and the Culture of Performance* (Stanford, 1993), p. 161.

7. Peter W. Williams, "The Varieties of American Medievalism," *Studies in Medievalism* 1, no. 11 (spring 1982): 8.

8. Bede, *A History of the English Church and People*, trans. Leo Sherley-Price, revised by R. E. Latham (New York, 1977), pp. 55–56.

9. Frantzen, *Desire for Origins*, p. 16.

10. Williams, "The Varieties of American Medievalism," p. 10.

11. F. N. Robinson, "Anniversary Reflections," Presidential Address delivered at the Twenty-Fifth Annual Meeting of the Medieval Academy, *Speculum*, 25 (1950): 494.

12. Philip Gleason, "American Catholics and the Mythic Middle Ages," in *Keeping the Faith American Catholicism Past and Present* (Notre Dame, 1987), p. 20. Unfortunately, space does not allow me to pursue the development of Catholic medievalism in North America, which, at least in its initial stages (before the Neo-Thomist revival) was conducted largely outside the mainstream of the academy. Gleason's article offers a sensitive and comprehensive discussion. See also John Van Engen, "The Christian Middle Ages as an Historiographical Problem," *American Historical Review* 91 (1986): 519–52. On the Romantic components of late-nineteenth-century medievalism as they affected both popular culture and historical writing see the excellent article by Robin Fleming, "Picturesque History and the Medieval in Nineteenth-Century America," *American Historical Review* 100 (1995): 1061–94. For an extremely insightful treatment of medievalism's antimodernist agenda see T. J. Jackson Lears, *No Place of Grace: Antimodernism and the Transformation of American Culture, 1890–1920* (reprint, Chicago and London, 1994).

13. William J. Courtenay, "The Virgin and the Dynamo: The Growth of Medieval Studies in North America 1870–1930," in Francis G. Gentry and Christopher Kleinhenz, eds., *Medieval Studies in North America Past, Present and Future* (Kalamazoo, Mich., 1982), p. 21, note 8.

14. Cited in *ibid.*, p. 10.

15. *Ibid.*, p. 5.

16. The judgment of his teaching offered by Courtenay, *ibid.*, p. 6.

17. See Patrick Geary, "Visions of Medieval Studies in North America," in John Van Engen, ed., *The Past and Future of Medieval Studies* (Notre Dame and London, 1994), pp. 51–52. Alternatively, however, Susan Mosher Stuard has argued that Adams was the first to focus on the "social balance" between the sexes, particularly in the family, as an important element in Europe's dynamic pattern of growth, a phenomenon disrupted by the processes of state building. In that sense, Adams was among the first medievalists to introduce a concern with the family, women, and gender as a counterpart to the history of state, a view with a potentially subversive edge for American historiography. See her "A New Dimension? North American Scholars Contribute Their Perspective," in Susan Mosher Stuard, ed., *Women in Medieval History and Historiography* (Philadelphia, 1987), p. 84. But

Stuard acknowledges that Adams failed to incorporate these concerns, like his concerns with cultural history, into his teaching, which was restricted to precisely the history of political reigns, wars, and administrative milestones whose centrality his writings might have contested. Adams is, however, an extremely complex figure, whose work does not fall easily into categories, being at once informed by some highly modernist tendencies (see, for example, the recent essay by Dorothy Ross, "Modernist Science in the Land of the New/Old," in Dorothy Ross, ed., *Modernist Impulses in the Human Sciences 1870–1930* [Baltimore, 1994], pp. 171–89), evident most especially in Adams's lifelong interest in science, as well as the better known antimodernist aspects of *Chartres* and the last three chapters of *The Education*. The bibliography on Adams is enormous, but a good starting place is J. C. Levenson, *The Mind and Art of Henry Adams* (Boston, 1957), together with the standard multivolume biography by Ernest Samuels, *The Young Henry Adams* (Cambridge, Mass., 1948); *Henry Adams: The Middle Years* (Cambridge, Mass., 1958); *Henry Adams: The Major Phase* (Cambridge, Mass., 1964).

18. Certainly, the most important writer of medieval history before Haskins was Henry Charles Lea (1825–1909), author of such works—to name only a few—as *Superstition and Force: Essays on the Wager of Law, the Wager of Battle, the Ordeal, Torture*, 4th ed. (Philadelphia, 1892); *A History of the Inquisition in Spain*, 4 vols. (New York, 1906–7); *History of Sacerdotal Celibacy*, 4th ed. (London, 1932) and *Materials toward a History of Witchcraft*, ed. Arthur C. Howland, 3 vols. (Philadelphia, 1939); *Torture* (Philadelphia, 1973). Lea, however, was not a professor but a publisher. Indeed, the first generation of American medievalists tended to be gentlemen scholars, whose private wealth funded their amateur historical scholarship. It is a tribute to Lea's industry and intelligence that he could produce such important work on such a massive scale while still functioning as a publisher in Philadelphia. Another important medieval scholar of the late nineteenth century, author of a widely circulated book, *Thirteenth, Greatest of Centuries* (New York, 1912), was James J. Walsh, a medical doctor who had trained under Rudolph Virchow in Germany. Although Walsh was an important contributor to the tradition of popular medievalism, the impact of his work lay largely outside academia. On Walsh see Gleason, "American Catholics and the Mythic Middle Ages," pp. 19 ff.

19. For details of Haskins's and Strayer's lives and early careers see Norman Cantor, *Inventing the Middle Ages: The Lives, Works, and Ideas of the Great Medievalists of the Twentieth Century* (New York, 1991), chap. 7, "American Pie, Charles Homer Haskins and Joseph Reese Strayer," passim. On Haskins see also Sally Vaughn, "Charles Homer Haskins (1870–1937)," in *Medieval Scholarship Biographical Studies on the Formation of a Discipline*, vol. 1, *History*, ed. Helen Damico and Joseph B. Zavadil (New York and London, 1995), pp. 169–84.

20. *Ibid.*, p. 252.

21. On Haskins's role in the formation of the Medieval Academy and *Speculum*, a role rather more restricted than Cantor intimates, see George R. Coffman, "The Medieval Academy of America: Historical Background and Prospect," *Speculum* 1 (1926): 5–18.

22. On Germany's influence on American historiography, due to the training that so many of the first generation of professional historians received there, see Jürgen Herbst, *The German Historical School in American Scholarship: A Study in the Transfer of Culture* (Ithaca, 1965).

23. Charles Homer Haskins, "European History and American Scholarship," *American Historical Review* 28 (1923): 215.

24. *Ibid.*, p. 18. Behind the profound passion of this statement lies, to be sure, an equally profound anxiety over just how marginal and irrelevant medieval history must

seem to most Americans. In 1971, Strayer openly articulated the threat underlying the American practice of medieval history, warning a new generation of students to whom he addressed his remarks that, without concerted effort, they were in danger of being “shoved into the back corner along with Sanskrit, Assyriology and other subjects” (i.e., all the *dead* languages, betraying the threat of nonbeing that always haunts the medievalist’s imaginary). For, Strayer reminded his young audience, “We should never forget our greatest danger: we began as antiquarians and we could end as antiquarians” (“The Future of Medieval History,” *Medievalia et Humanistica*, n.s. II [1971]: 179). The insistence on continuity and relevance marked the American appropriation of the medieval past for decades. In his presidential address on “Humanistic Studies and Science” on the occasion of the fifth annual meeting of the Medieval Academy, John Matthews Manly sounded its plea once again, imploring that “the infinitely various and fascinating period we roughly call the Middle Ages must not be neglected. It lies close to us. In it arose many of our most important institutions. Our social life, our customs—our ideals, our superstitions and fears and hopes—came to us directly from this period; and no present-day analysis can give a complete account of our civilization unless it is supplemented by a profound study of the forces and forms of life, good and evil, which we have inherited from it” (*Speculum* 5 [1930]: 250). As late as 1963, S. Harrison Thomson, surveying the field, echoed Haskins’s sentiment by declaring that “The Middle Ages are early American history and they should be so presented.” See S. Harrison Thomson, “The Growth of a Discipline: Medieval Studies in America,” in Katherine Fischer Drew and Floyd Seyward Lear, eds., *Perspectives in Medieval History* (Chicago, 1963), p. 17.

25. Coffman, “The Medieval Academy of America,” p. 17.

26. Charles Homer Haskins, *The Renaissance of the Twelfth Century* (reprint, New York, 1964), pp. vii–viii.

27. Haskins’s appreciation of the importance of science to medievalism’s modernist agenda was implemented in his important research on medieval science (see his *Studies in the History of Mediaeval Science* [Cambridge, Mass., 1924]). This aspect of Haskins’s influence was continued and amplified by Lynn White’s investigations into the history of technology, beginning in the 1950s, the effects of which were, in John Van Engen’s helpful phrasing, “to rewrite medieval culture to approximate American dynamism.” See his “An Afterword on Medieval Studies; or, the Future of Abelard and Heloise,” in Van Engen, ed., *The Past and Future of Medieval Studies*, p. 414.

28. The term was popularized by Wallace K. Ferguson, *The Renaissance in Historical Thought: Five Centuries of Interpretation* (Boston, 1948).

29. Haskins, “European History and American Scholarship,” p. 224; 226.

30. Space does not allow for a full discussion of the impact of German positivism and philology on Medieval Studies in America, but its perduring effects would be difficult to overestimate. For a discussion of this from various points of view see, among others, Frantzen, *Desire for Origins*, passim; Patterson, *Negotiating the Past*, passim; idem, “Critical Historicism and Medieval Studies”; and especially the wide range of essays in the collective volume edited by R. Howard Bloch and Stephen G. Nichols under the title *Medievalism and the Modernist Temper* (Baltimore, 1996). The author would like to thank the editors for allowing her to read the book in manuscript. Especially useful essays in that volume are David Hult, “Gaston Paris and the Invention of Courtly Love,” and Stephen G. Nichols, “Modernism and the Politics of Medieval Studies.” On the alliance of philology with French and German national movements see also R. Howard Bloch, “Naturalism, Nationalism, Medievalism,” *Romanic Review* 76 (Nov. 1985): 341–60, and Hans Ulrich Gumbrecht, “Un Souffle d’Allemagne ayant passé: Friedrich Diez, Gaston Paris and the Genesis of National

Philologies," *Romance Philology* 40 (Oct. 1986): 1–37. It is interesting that American medievalists' allegiance to positivism survived World War I intact, despite the well-known "crisis of historicism" in Germany in the immediate postwar years, echoed in America primarily in the work of Charles Beard and Carl Becker. Indeed, Peter Novick has demonstrated that, among American historians, medievalists were most resistant to the currents of relativism that surfaced after World War I. Whereas Carl Becker and Charles Beard used the occasion of their presidential addresses in 1931 and 1933 respectively to articulate their relativist doctrines—Becker in his "Everyman His Own Historian," *American Historical Review* 37 (1932): 221–36; Beard in "Written History as an Act of Faith," *American Historical Review* 39 (1934): 219–31—Charles McIlwain devoted his presidential address of 1936 to attacking Beard's of three years earlier, in order to uphold the premises of scientific objectivity against his colleagues' relativism. See his "The Historian's Part in a Changing World," *American Historical Review* 42 (1937): 207–24. Medievalists were not alone in defending the "noble dream" of objectivity, but they were universally on its side in the debate that erupted. On this see Peter Novick, *That Noble Dream: The "Objectivity Question" and the American Historical Profession* (Cambridge, Eng., 1988), esp. chap. 9. Also useful is John Higham, *History Professional Scholarship in America* (Baltimore and London, 1965).

31. According to William J. Courtenay, the most striking effect of the war on Americans was to redirect both attention and training away from Germany to France, Belgium, and England ("The Virgin and the Dynamo," p. 14ff). One consequence of this shift was to inaugurate a small but powerful following in America of the Belgian historian Henri Pirenne, among whose most famous students were Carl Stephenson, James Bruce Ross, and Bryce Lyon. Another was the virtual extinction of German history as a field in North America, a phenomenon that remains true down to the present day. On this latter phenomenon see Patrick J. Geary, "Medieval Germany in America," *Annual Lectures 1990* (Washington, D.C.: The German Historical Institute, 1991); Edward J. Peters, "More Trouble with Henry: The Historiography of Medieval Germany in the Angliterate World, 1888–1995," *Central European History* 28 (1995): 47–72; and aspects of Giles Constable, "The Many Middle Ages Medieval Studies in Europe as Seen from America," in Jacqueline Hamesse, ed., *Bilan et Perspectives des Etudes Médiévales en Europe* (Louvain-la Neuve, 1995), pp. 1–22.

32. Here, again, Charles McIlwain provides the most interesting example. In contrast to his firm maintenance of scientific historiography and objectivity in his 1936 presidential attack on Beard's relativism, already before the end of World War II McIlwain found himself responding to political events via a revision of his earlier understandings of Roman law. Thus, in an article entitled "Medieval Institutions in the Modern World" (*Speculum* 16 [1941]: 275–83), McIlwain allowed present events in Germany to reorient completely his notion of the place of Roman law in Western constitutionalism, arguing that "if we find the outcome of the Germanic origin of our institutions in the barbarous tribal orgy and the fantastic tribal history of the Germany of today, we may well begin to wonder if we have not been overdoing our own notions both of the continuing importance of our Germanic origins and of the accuracy, or at least the adequacy of the von Maurers of yesterday or of the von Gierkes of today." In place of his insistence in his presidential address that objectivity was not only possible but incumbent upon the historian, and that it could be maintained in the face of the sort of presentist concerns that both Beard and Becker stressed, McIlwain now confessed that "for myself it has been the tribal excesses of present-day Germany which, as much as anything else, have led me to question the group theory of von Gierke's *Genossenschaftsrecht* either as an explanation of medieval life or as a principle of practical politics" (pp. 279–80). Moreover, McIlwain opined, the Nazi repudiation of Roman law suggested that medievalists had greatly overemphasized the despotic character

of that great legal corpus and had, conversely, greatly underrated the "importance of Roman constitutionalism in the early development of our own" (p. 278). This domestication and democratization of what had earlier been seen as the absolutist tendencies of Roman law, begun in an American context by McIlwain, was to gain powerful allies from the German émigré community of medievalists, among them Stephen Kuttner, Walter Ullmann (in England), and especially Ernst Kantorowicz, whose students included Robert Benson and Ralph Giesey. See also the essays of Gaines Post, collected in his *Studies in Medieval Legal Thought* (Princeton, 1964). A parallel group of works has emanated, as the anonymous reader of this volume for Princeton University Press helpfully pointed out, from Brian Tierney and his students, who devoted themselves to "the study of the relationship between medieval constitutionalism and the origins of the modern liberal state." McIlwain's 1941 article is notable for its prescient awareness of the impact that the war would have on American scholarship.

33. Norman Cantor has persuasively argued that Haskins's achievement as a medieval historian lay in applying the basic tenets of Wilsonian progressivism to the study of medieval history, leading him to insist on the beneficial consequences of centralized power in the hands of an educated and professional elite, whether medieval or modern. See *Inventing the Middle Ages*, p. 249.

34. As in the title of his book, Joseph Reese Strayer, *On the Medieval Origins of the Modern State* (Princeton, 1970).

35. See Cantor, *Inventing the Middle Ages*, p. 260.

36. Published by The Medieval Academy of America (Cambridge, Mass.), in 1932.

37. Cantor, *Inventing the Middle Ages*, p. 258.

38. Joseph Reese Strayer, "Philip the Fair—a 'Constitutional King,'" *American Historical Review* 62 (1956): 18–32.

39. In particular by Bryce Lyon, "What Made a Medieval King Constitutional," in *Essays in Medieval History Presented to Bertie Wilkinson* (Toronto, 1969). I am indebted to Professor John W. Baldwin for this reference.

40. See Joseph Reese Strayer, *Studies in Early French Taxation* (Cambridge, Mass., 1939), *passim*.

41. The first was published in *Studi in onore di Gino Luzzatto*, 1 (Milan, 1949), pp. 289–96; reprinted in John Benton and Thomas Bisson, eds., *Medieval Statecraft and the Perspectives of History: Essays by Joseph Reese Strayer* (Princeton, 1971), pp. 12–27; the latter in Theodore K. Rabb and Jerrold E. Siegel, eds., *Action and Conviction in Early Modern Europe: Essays in Memory of E. R. Harbison* (Princeton, 1969), pp. 3–19, reprinted in Benton and Bisson, eds., *Medieval Statecraft and the Perspectives of History*, pp. 300–314.

42. Joseph Reese Strayer, "The Fourth and the Fourteenth Centuries," *American Historical Review* 77 (1972): 1–14.

43. Joseph Reese Strayer, "The Laicization of French and English Society in the Thirteenth Century," *Speculum* 15 (1940): 76–86, reprinted in Benton and Bisson, eds., *Medieval Statecraft and the Perspectives of History*, pp. 251–65.

44. Cantor, *Inventing the Middle Ages*, p. 254.

45. The term *postmodernism* first gained renown with the publication of Jean François Lyotard's *La condition postmoderne: rapport sur le savoir* (Paris, 1979) in 1979 but probably was not widespread in American historiography until the translation of Lyotard's book in 1984. On this, see William D. Paden, "Scholars at a Perilous Ford," in William D. Paden, ed., *The Future of the Middle Ages Medieval Literature in the 1990s* (Gainesville, 1994), p. 8.

46. Eric L. Santner, *Stranded Objects: Mourning, Memory, and Film in Postwar Germany* (Ithaca, 1990), p. 51.

47. On the negative appeal of the Middle Ages see Paden. "Scholars at a Perilous Ford," p. 21.

48. For a fuller discussion of much of what follows see my "History, Historicism and the Social Logic of the Text in the Middle Ages," *Speculum* 65 (1990): 59–86, and my response to the debate on "History and Postmodernism" it generated in *Past and Present* 135 (1992): 194–208.

49. To some extent, this kind of work might be thought to derive from Marc Bloch's emphasis on *mentalité*, an initially neglected aspect of the "Annales paradigm," but there has been very little work in specifically medieval history (in contrast to early modern history) in America that takes its primary impetus from the Annales school. Not only does there not exist an identifiable American school dedicated to the study of medieval *mentalités*, neither can the profound changes represented by the rise of discursively oriented work really be traced back to the Annales, however compatible the Annalist emphasis on *mentalité* might at first seem to be with it. In truth, they employ quite different views of language, hence of the nature of medieval textuality and the uses to which it can, and should, be put.

50. Eugene Vance, "Semiotics and Power: Relics, Icons and the *Voyage de Charlemagne à Jérusalem et à Constantinople*," in Marina S. Brownlee, Kevin Brownlee, and Stephen G. Nichols, eds., *The New Medievalism* (Baltimore, 1991), p. 227.

51. The earliest work in medieval women's history, such as that of Jo Ann McNamara and Suzanne Wemple, focused on the ways that historical scholarship had occluded women's historical presence and sought to restore them to view. See, for example, the 1981 book by Suzanne Wemple, *Women in Frankish Society: Marriage and the Cloister, 500 to 900* (Philadelphia, 1981), and McNamara's essay in her edition of papers from the Fifth Berkshire Conference on the History of Women, *Women and the Structure of Society: Selected Research from the Fifth Berkshire Conference on the History of Women* (Durham, N.C., 1982), and as well the collected works of this "Columbia school" of women's history in *Women of the Medieval World: Essays in Honor of John H. Mundy* (Oxford, 1985). The basic impulse of this early work was to make women visible upon the historical stage, but as Wemple's and McNamara's own developments indicate, an exclusive focus on women soon became normative. See, for example, McNamara's later books, *Sisters in Arms: Catholic Nuns through Two Millennia* (Cambridge, Mass., 1996) and *Sainted Women of the Dark Ages* (Durham, N.C., 1992). Interestingly, McNamara's feminism has now carried her into a study of masculinity as well, in her recent work *Medieval Masculinities: Regarding Men in the Middle Ages* (Minneapolis, 1994). For an overview of these developments in the field of medieval history see Susan Mosher Stuard, "A New Dimension? North American Scholars Contribute their Perspective," in Stuard, ed., *Women in Medieval History*, pp. 81–99. See also the recent special volume of *Speculum* 68 (1993) dedicated to women's history, now published as Nancy Partner, ed., *Studying Medieval Women, Sex, Gender, Feminism* (Cambridge, Mass., 1993). Feminist scholarship on medieval women is now far too extensive to cite comprehensively, but an example of this sort of work is Penny Shine Gold, *The Lady and the Virgin: Image, Attitude and Experience in Twelfth-Century France* (Chicago, 1985).

52. Here the work of Caroline Bynum has been decisive, especially her *Holy Feast and Holy Fast: The Religious Significance of Food to Medieval Women* (Berkeley, 1987) as well as her most recent works, *Fragmentation and Redemption: Essays on Gender and the Human Body in Medieval Religion* (New York, 1991) and *The Resurrection of the Body in Western Christianity, 200–1336* (New York, 1995).

53. See, for example, the collective article by E. Jane Burns, Roberta Krueger, and Helen

Solterer, "Feminism and the Discipline of Old French Studies," in Bloch and Nichols, *Medievalism and the Modernist Temper*, passim.

54. An extremely useful survey of these changes is Michael Kammen's "The Historian's Vocation and the State of the Discipline in the United States," in Michael Kammen, ed., *The Past before Us: Contemporary Historical Writing in the United States* (Ithaca, 1980), pp. 19–46; see also the essay on medieval historiography by Karl Morrison, "Fragmentation and Unity in American Medievalism," in *ibid.*, pp. 49–77.

55. Paul Freedman, "The Return of the Grotesque in Medieval Historiography," in Carlos Barros, ed., *Historia A Debate: Medieval* (Santiago de Compostella, 1995): 9–19.

56. *Ibid.*, p. 16.

57. Michel Foucault, *The Archeology of Knowledge* (New York, 1972), p. 5

58. On the significance of treating texts as "monuments" rather than "documents" see Frantzen, *The Desire for Origins*, which seeks to apply these principles to the study of Anglo-Saxon literature.

59. Very recent examples of work done in this vein are Joan Cadden, *The Meaning of Sex Differences in the Middle Ages* (Cambridge, Eng., 1994); John W. Baldwin, *The Language of Sex: Five Voices from Northern France around 1200* (Chicago, 1994); E. Jane Burns, *Bodytalk: When Women Speak in Old French Literature* (Philadelphia, 1993). There is a huge literature on medieval sexuality, beginning with the work of Vern L. Bullough, *Sexual Practices and the Medieval Church* (Buffalo, 1982), and James Brundage, *Law, Sex, and Christian Society in Medieval Europe* (Chicago, 1987).

60. See the works of Caroline W. Bynum cited in note 52.

61. R. I. Moore, *The Formation of a Persecuting Society: Power and Deviance in Western Europe, 950–1250* (Oxford, 1987); and *idem*, *The Origins of European Dissent* (New York, 1985); *idem*, *The Birth of Popular Heresy* (New York, 1976). John E. Boswell, "Jews, Bicycle Riders and Gay People: The Determination of Social Consensus and Its Impact on Minorities," *Yale Journal of Law and the Humanities* 1 (1989): 205–28. See also the series of works by Jeffrey Russell, *Dissent and Order in the Middle Ages: The Search for Legitimate Order* (New York, 1992); *idem*, *Lucifer: The Devil in the Middle Ages* (Ithaca, 1984); *idem*, *Religious Dissent in the Middle Ages* (New York, 1971). For a general bibliography see Carl Berkout, *Medieval Heresies: A Bibliography, 1960–1979* (Toronto, 1981). Also relevant is the work of Edward J. Peters, *Heresy and Authority in Medieval Europe* (Philadelphia, 1988); *idem*, *Inquisition* (New York, 1988); *idem*, *Torture* (New York, 1985); *idem*, *The Magician, the Witch and the Law* (Philadelphia, 1978); *idem*, with Alan Kors, *Witchcraft in Europe* (Philadelphia, 1972).

62. Norman Cohn, *Europe's Inner Demons: An Enquiry Inspired by the Great Witch-Hunt* (New York, 1975).

63. See William Chester Jordan, *The French Monarchy and the Jews: From Philip Augustus to the Last Capetians* (Philadelphia, 1989), and his *Women and Credit in Pre-industrial and Developing Societies* (Philadelphia, 1993), which in part concerns financial transactions (loans) between women and Jews.

64. The work of the late John Boswell is critical here, especially his two books, *Christianity, Social Tolerance and Homosexuality: Gay People in Western Europe from the Beginning of the Christian Era to the Fourteenth Century* (Chicago, 1980) and *Same-Sex Unions in Premodern Europe* (New York, 1994).

65. See the work of Robert Chazan, *European Jewry and the First Crusade* (Berkeley, 1987); *idem*, *Daggers of Faith: Thirteenth-Century Christian Missionizing and Jewish Response* (Berkeley, 1989); *idem*, "The Representation of Events in the Middle Ages," *History and*

Theory 27 (1988): 40–55; and of Ivan Marcus, “History, Story and Collective Memory: Narrativity in Early Ashkenazic Culture,” *Prooftexts* 10 (1990).

66. See Eleanor Searle, *Predatory Kinship and the Creation of Norman Power* (Berkeley, 1988); and Thomas N. Bisson, “The ‘Feudal Revolution,’” *Past and Present* 142 (1994): 6–42.

67. Thus Lee Patterson has specifically advocated the adoption of an ironic mode of history as that best adapted to a postmodernist treatment of the medieval past. See his “On the Margin,” *passim*.

68. Freedman, “The Return of the Grotesque,” p. 9.

69. Van Engen, “An Afterword on Medieval Studies,” p. 414.

70. Santner, *Stranded Objects*, p. 7.

Between Whig Traditions and New Histories:
American Historical Writing
about Reformation and
Early Modern Europe

PHILIP BENEDICT

IN ITS DEPICTION of professorial paladins of many nationalities jetting from conference to conference across oceans and time zones, David Lodge's academic novel *Small World* neatly captures one of the essential features of the contemporary world of scholarship—the dramatically increased pace of international scholarly cross-fertilization and migration. Perhaps no field of American historical writing has felt the effects of this more than the study of European history from 1500 to 1789. This period has been the focus of much of the most innovative work of the major European movements of “new history” in the past generations: the *Annales* school, the group of English historians around *Past and Present*, and the Italian microhistorians around *Quaderni Storici*. In the years since the second great expansion of the American historical profession began amid the postwar educational boom of the late fifties and sixties, the growth of support for international research, cheap transatlantic airfares, several consecutive decades of a strong dollar, and the multiplication of international conferences and exchanges all combined to increase commitment to archival research and to transform the ambitions and horizons of American scholars in this field. So many have produced archive-based monographs of a depth and sophistication comparable to those written in Europe that the prominent specialist in French history who advised his colleagues in 1958 that Americans could not compete in this domain and that they should concentrate instead on synthesizing European archival work graciously acknowledged in 1991 that he had been proved wrong.¹ Meanwhile, the relative wealth and openness of the American university system has drawn to the United States so many prominent European early modernists—Heiko Oberman, Lawrence Stone, Carlo Ginzburg, J. H. Elliott, Simon Schama, to name just a few—that it is hard even to know where to draw the boundaries of “American” scholarship.² A few native scholars have assumed a position among the most influential historians anywhere in the world. Many others are now interlocutors in international discussions on an equal standing with their counterparts in the various countries of Europe. The problems to which early modern historians working in the United States address themselves, as well as the methods they

employ, are as much those of the different European historiographic traditions with which they interact as those they share with domestic colleagues in other fields.

For all this increased internationalization of recruitment and perspectives, long-established curricular and organizational patterns nonetheless continue to lend a distinctive configuration to American research on this period of European history. Despite a growing tendency for all who work in this field to conceive of themselves as “early modernists,” American specialists in the history of this era subdivide themselves into several distinct, if occasionally overlapping, communities of discourse. For those concerned with the European continent, teaching responsibilities divide specialists in the early part of the period from specialists in its later centuries, with the study of the Reformation defining the central focus of the initial period—a vestige of the long-standing emphasis within the American teaching curriculum on “Ren-Ref.” By contrast, the period of continental European history from 1600 to 1789 has always lacked a clear identity or scholarly organizations similar to those which exist for the sixteenth century. English history from the sixteenth through the eighteenth centuries has meanwhile always constituted a field separate unto itself, one whose self-definition around the dubious dichotomization between Britain and Europe imparts to it an unusually high degree of insularity in its preoccupations with its particular debates and methods. Far smaller groups of early modernists also devote their attention to the history of science and—rare indeed—to Jewish history, each of which again is conventionally defined as constituting a separate field.

Despite the recent adoption of new methods and new problems under the influence of innovative historiographic currents both domestic and European, certain long-standing preoccupations still attract the attention of most people working on the seventeenth and eighteenth centuries. Political history, broadly understood, retains pride of place. Intellectual history, especially the genealogy of secular rationalism, also remains an enduring concern. Continuity may also be observed in the fact that the advance of specialization and the sheer increase in the work devoted to the period has brought no perceptible alteration of the long-standing concentration on the history of just a few countries within Europe, especially England and France. The growth of aspirations to produce archivally based studies of a quality comparable to the best European research and the advance of topical and national specialization have led fewer American historians to attempt the interpretive syntheses on a European scale that David Pinkney considered the finest products of the previous generation. With some distance, however, it can be seen that most American research about this period continues to be related to certain grand themes that have long defined its significance in the minds of American historians. Supplemented by some new big stories introduced in the past generation, these themes continue to structure most classroom instruction about this period and to suggest many of the topics deemed worthy of research. In this respect, these fields contrast sharply with the current situation in Renaissance history, as described by Anthony Molho in chapter 13.

THE DEEP STRUCTURES

Thirty years ago, Leonard Krieger accurately highlighted two particularly important influences on the shape of American historical scholarship about Europe. The first was what he called the “predominance of the undergraduate teaching function” in American academic life. American history professors are responsible for teaching large chronological or thematic swatches of the European past. The experience of having to create, year after year, convincing, integrated accounts of this subject for previously uninitiated undergraduates draws them toward a relatively high level of generalization, attracts their attention to certain possible objects of study, and obscures others. The second influence was the understandable attraction the first American historians of this era had for those aspects of the European past that seemed either to anticipate elements of American history, culture, and political traditions, or to define the distinctive features of American history by revealing what it was not. John Lothrop Motley’s brave little Holland fighting the first great war of independence and John William Draper’s and Andrew Dickson White’s centuries-old “warfare between science and religion” exemplify the former.³ William H. Prescott’s imperial Spain condemned to decline because of its intolerant Catholicism and tyrannical government and Henry Charles Lea’s Catholic Church of the Inquisition, auricular confession, clerical celibacy, and other blendings of superstition and force illustrate the latter.

The great liberal historians of nineteenth-century Europe also shaped the connection that educated Americans established with the European past. The required surveys of postclassical history that were a standard part of the curriculum at many colleges by the middle of the nineteenth century assigned works such as Guizot’s *History of Civilization* (a staple) and Hallam and Stubbs on English constitutional history.⁴ Within this context, the vision that nineteenth-century liberal historiography presented of the Reformation as a central episode in the emancipation of the human mind commended this subject for particular attention. For the seventeenth and eighteenth centuries, the central tradition of Whig historiography stressed the juxtaposition of English and French constitutional developments.⁵

The conservative evolutionism of America’s first generation of professional historians reinforced concern with such topics. As autonomous departments of history took shape between 1885 and 1910 and the first wave of professional expansion produced a varied menu of specialized courses, a substantial fraction—often more than half—of the menu was devoted to medieval and early modern Europe. Each college curriculum developed in its own manner, but the recurring staples of instruction for the period from 1300 through 1815 were those aspects of the European past considered to have either a clear genetic connection to American political and religious institutions and traditions, to illuminate by contrast the character of the American Revolution, or to be of larger significance in the great saga of gradual human emancipation: the Renaissance and Reformation, English

history, and the French Revolution. Continental European history from roughly 1600 until that point in the later eighteenth century when the courses on the French Revolution picked up their story was covered on a far more selective and aleatory basis. Certain courses made particularly clear the genetic connections that were seen between the elements of the early modern past that the curriculum emphasized and American history; this is seen most graphically in the course that Herbert Darling Foster taught at Dartmouth for many years: *The Puritan State in Geneva, England, and Massachusetts Bay*.⁶

The configuration of instruction about late medieval and early modern European history has changed only modestly since the early twentieth century. To be sure, as history departments grew, so did the number and range of courses about this period. Expansion was greatest between the late 1950s and 1970 and chiefly involved greater investment in the previously neglected seventeenth and eighteenth centuries. Courses on the intellectual and the economic history of the era were also introduced early in the century in certain universities; the scientific revolution became a staple offering with the rise of the history of science; and recently many departments have begun to offer courses on women's history in this period. Still, in the absence of strong student demand or an evident national or political interest to be served by curricular expansion in this area, the initial heavy investment in the medieval and early modern European past has faded, as growth in course offerings about these centuries has lagged far behind that in American, more recent European, or non-Western history. Course offerings on the years 1600–1789 still diverge significantly from university to university, and for significant stretches of the postwar period, prestigious institutions were content to teach no courses at all on continental Europe between the Reformation and the onset of the French Revolution. Meanwhile, the pairing of the Renaissance and Reformation proved a hardy perennial, allowing instructors to ring a variety of changes on either the contrast between the secularizing, rationalist aspects of the Renaissance and the biblicism of the Reformation or the continuities between the humanist recovery of letters and the Protestant recovery of the gospel. Today the pairing carries less conviction for most specialists, and *The Age of the Renaissance* and *The Age of the Reformation* are most often taught as separate courses. But few who teach courses on the fifteenth or sixteenth century have dared to abandon the advertising power that these labels retain.

One result of these patterns was a long-standing tendency for early modernists in America to concentrate much of their attention on the history of the sixteenth century. The chronological distribution of articles in the most prestigious American and foreign journals shows that through the 1960s American scholars published more about the sixteenth century than about either of the subsequent two (see Table 1). This has now changed, but comparison with the situation in many European countries (e.g., France) might still suggest an unusually high level of concern with the sixteenth century.

The focus on the Renaissance and Reformation has also bred a covey of professional institutions, with their attendant scholarly journals: the American Society for Reformation Research (incorporated in 1947), the Renaissance Society of

TABLE 1
Chronological Focus of Articles Devoted to the Period 1500–1789 by
American Scholars in Four Major Historical Journals, 1900–1990

	1900–10	1930–40	1960–70	1980–90
16th century	10 (40%)	18 (37%)	22 (40%)	21 (20%)
17th century	8 (32%)	17 (35%)	18 (33%)	32 (30%)
18th century	7 (28%)	14 (29%)	15 (27%)	53 (50%)

Note: Based on a survey of articles by scholars affiliated with North American universities appearing in *The American Historical Review* (AHR), *The Journal of Modern History* (JMH), *Annales* (An.), and *The Historical Journal* (HJ). Articles have been classified with reference to their chief century of focus. Those covering a sweep of several centuries have been omitted.

America (founded in 1954), and the Sixteenth Century Studies Council (established in 1972). No comparable institutions have developed for the seventeenth century or for early modern European history as a whole, while the Society for Eighteenth-Century Studies (founded in 1969), although attracting the participation of some historians, is more strongly dominated by scholars of literature and art. Historians working on the later centuries find their chief professional peer groups in the many associations devoted to the history of individual European countries or topical specializations, such as the Society for French Historical Studies, the Council for British Studies, or the Social Science History Association.

REFORMATION HISTORY

The construction in America of the distinct field of Reformation history and its precocious institutionalization in the history curriculum did not result from just the prominence that nineteenth-century liberal historiography accorded the rise of Protestantism in its saga of the advance of liberty. The centrality of the period for the historical self-definition of so many Protestant churches also commended the subject to the attention of the Protestant-dominated academic culture of the late nineteenth and early twentieth century. To this day, a powerful impetus attracting scholars to this subject remains the concern of Christian believers to explore the roots of their diverse traditions. Important work continues to be carried out not simply in the history departments of research universities but also within divinity schools, departments of religion, and small denominational colleges. The distinctive configuration of American religious life has consequently left a clear impress on this branch of American historiography. Its governing concerns and assumptions have changed substantially since the day when Protestantism's special contribution to the making of the modern world was axiomatic. The past generation has brought particularly dramatic transformations. Yet the field remains a point of encounter between agnostics and those attached to a specific religious vision or heritage. Considerable creative tension between different outlooks, methods, and foci of concern has resulted.

In the early years of the American historical profession, relations were strained between the more secular-minded Reformation historians and those who approached the subject with strong religious sensibilities. Thanks largely to the efforts of the energetic Philip Schaff (1819–93), the great pioneer of church history in America, ecclesiastical historians formed their own learned society in 1888, the American Society of Church History. The society decided in 1896 to merge with the American Historical Association, resolving that “Church history is only a part of general history.” But ten years later its members reestablished the organization, for they felt marginalized within the AHA and had trouble getting their papers published in the larger association, whose officials feared that printing excessively narrow research about the history of Christian doctrine or institutions might violate the separation of church and state and endanger the association’s government support.⁷ In the meantime, an aggressively secular historiography, committed to rescuing the subject from what were perceived to be the blinkered perspectives of the church historians, developed among the ranks of the “New Historians.” James Harvey Robinson proclaimed in 1903 that the field stood on the brink of a new understanding of the Reformation that would highlight its social, political, intellectual, economic, and institutional changes. His students investigated early Protestant social welfare policy and pioneered the application of Freudian analysis to Martin Luther’s biography.⁸ The substantial attention that the New Historians devoted to the history of science also gave them a heightened sense of the distance between the Reformation era and the contemporary world. It was in these circles and this generation that American historians assimilated for the first time the concept of the scientific revolution, with its identification of the critical turning point in European thought between the late sixteenth and the eighteenth centuries.⁹ The synthetic *Age of the Reformation* (1920) by Robinson’s leading student, Preserved Smith, set the Reformation amid a far broader range of economic, political, and intellectual contexts than comparable earlier works. The book was also notably devoid of pronouncements about the superiority of the Protestant nations over the Catholic and of statements about the importance of the Reformation in “the permanence and progress of civilization” such as those found fifty years earlier even in the work of George Park Fischer, one of the early ecclesiastical historians who most eagerly embraced the ideal of value-neutral scholarship.¹⁰

Between the 1930s and the 1960s, the tension that had previously characterized the relations between secular and church-minded historians largely dissipated. Broad currents within the historical profession attenuated the emphasis that the New Historians had accorded economic forces and enhanced appreciation for the autonomous force of ideas. The discovery of Luther’s early lectures promoted within Protestant theological circles a vision of the young reformer as a great existential hero of faith, and this vision stimulated renewed appreciation of the potential relevance of Reformation thought for contemporary society. Until ongoing examination of the critical early texts led the majority of experts to shift in the 1960s and 1970s toward a later dating of Luther’s critical “tower experience,” this vision also pictured a reformer who had achieved his critical theolog-

ical insights before the press of events forced him reluctantly into opposition to Rome. All this lent powerful support to the view that the Reformation was in origin a theological revolution, incomprehensible without a good understanding of the history of Christian doctrine.¹¹

Until the 1960s, American Reformation scholarship focused overwhelmingly on the Protestant side of the story. Most elite research universities remained tied to a liberal Protestant outlook well into the twentieth century, with few Catholics or Jews on the faculty until the postwar years. Catholic higher education was self-enclosed and parochial; the limited amount of historical scholarship carried out within its confines centered overwhelmingly on the Middle Ages, which were seen as the great age of Catholic faith and learning, or the Catholic contribution in American history. When John Dolan surveyed "Church History in England and America in the Nineteenth and Twentieth Centuries" for the 1965 *Catholic Handbook of Church History*, the discussion of American research required less than a page and did not cite a single work on the era of the Counter-Reformation.¹²

Within the history of Protestantism, a broad variety of subjects attracted the attention of American scholars, a consequence of the exceptional range of Protestant denominations found on American soil. Schaff's work surveyed all the major churches to emerge from the magisterial Reformation and can be seen as the attempt of a pioneer Protestant ecumenicist to understand and appreciate the origins and points of difference between the many different creeds he encountered as an immigrant from Germany to America. Other historians of theology would follow the trail that Schaff had blazed from Germany to America, notably Wilhelm Pauck in 1925. Until the Nazi era, the continuing prestige of German theological learning also lured many American church historians to Germany for part of their education. In consequence, the center of gravity of American Reformation scholarship increasingly became the politics of the Reformation in Germany and the thought of Martin Luther—a situation reinforced after 1945 by the agreement of the American Society of Reformation Research to publish the *Archiv für Reformationsgeschichte* jointly with the German Verein für Reformationsgeschichte. Yet a country with as strong a Calvinist heritage as the United States could hardly ignore the Reformed tradition. In the first part of the twentieth century, important studies were devoted to Zwingli and Zurich, to Calvin and Geneva, and to the French Wars of Religion.¹³ In the generation of Perry Miller, William Haller, and M. M. Knappen, Americans distinguished themselves in the study of Old English as well as New England Puritanism.¹⁴ Above all, the presence on American soil of many churches that traced their descent to the "left wing of the Reformation," as well as the constitutionally mandated separation of church from state that has led American scholars to view separatist groups positively as precursors in the struggle for religious liberty, produced unusual concern with the "radical Reformation."

As the most exacting critics, such as Pauck, have observed, it was only in this field of study that American researchers prior to the 1960s made truly substantial contributions to international Reformation scholarship. Several holders of the

most prominent chairs in ecclesiastical history during the interwar and immediate postwar decades devoted much of their original research to exploring the sectarians and dissenters of the sixteenth and seventeenth centuries, notably Yale's Roland Bainton and Harvard's George Huntston Williams.¹⁵ But nobody was more influential than the Mennonite Harold S. Bender (1897–1964), whose efforts to accumulate the materials for, and to promote research about, Anabaptist history made Goshen College in Indiana an internationally reputed center for the study of the subject. In his major publications, Bender depicted Anabaptism as springing from a single source in Zurich, where a small band of people dared to carry the Reformation principle of *sola scriptura* to its full, logical, pacifist consequences, from which the magisterial reformers shrank back out of fear and calculation. It was a depiction that offered an inspiring, historically based sense of identity for America's Mennonites, who had just created the institutions of higher learning long characteristic of other Protestant denominations and were struggling to come to grips with the wider world of modern historical knowledge and Biblical scholarship that this implied. At the same time, Bender's work posed a sharp challenge to the historical self-understanding of the mainline Protestant confessions and a powerful stimulus for further research.¹⁶ The expansion in knowledge about the radical Reformation promoted so energetically by Bender and others rescued the views of a wide range of groups and individuals formerly dismissed as fanatics by earlier church historiography. It also helped to reveal the full richness and variety of the reform programs generated amid the ferment of the early Reformation and drew attention to the political and social dynamics that promoted the institutionalization of certain visions of church reform and the marginalization of others. In so doing, it effected one of the central transformations of twentieth-century Reformation historiography.¹⁷

From the late 1960s onward, the field began to change dramatically. These years also saw one of the most internationally influential of all American historians emerge from within it: Natalie Zemon Davis.

Two central trends within historical scholarship in the past generation have been the expansion of historians' vision to encompass far more securely than previously the entire population of the place and period under study, and the shift within this expanded field of vision from an emphasis on the material conditions of life to an emphasis on culture. It does not seem entirely fortuitous that the scholar recognized as the most sophisticated and influential American trailblazer in the exploration of the culture of ordinary men and women should have emerged from Reformation history, where so much emphasis had already come to be placed on the need to respect the force and integrity of theological systems. But the personal intellectual trajectory that led Davis through the field was anything but ordinary. A secular Jew, she was drawn to the study of "Ren-Ref" as a student in the late 1940s at Smith College by an inspiring undergraduate teacher, Leona Gabel; by the still powerful belief that the origins of the modern world were to be found in the period; and by the intellectual excitement then being generated in the field by such figures as Hans Baron and Paul Kristeller.¹⁸ Her engagement with radical politics and Marxism led her first to study the material-

ist philosophers of sixteenth-century Italy, then, for her Ph.D., the Protestant printing workers of sixteenth-century Lyon. To study the latter was to engage with the work of Henri Hauser, the great French pioneer of labor history whose 1899 interpretation of the early Protestant movement as the cause of journeymen alienated by the advance of capitalism and the closure of access to master status was still the most forceful social interpretation of the Reformation. Davis's archival research into the identity of Lyon's Protestants revealed that the guild masters and journeymen did not line up on opposite sides of the religious question, and indeed that no clear divisions of economic interest could predict who joined the Reformed Church and who remained Catholic. Her work did show, however, that such features of social experience as literacy, migration, and individual craft traditions and identities appeared to correlate with religious choice.

From 1952 to 1959, Davis was refused a passport by the State Department because of allegations of Communism against her and her husband, who was blacklisted and jailed for invoking the First Amendment before the House Un-American Activities Committee. During these years she had to set aside archival research in Europe in favor of reading about matters relevant to her subject in American rare book rooms. When her most important articles began to appear from the mid-1960s onward, they deployed an exceptional range of source materials, both archival and printed, in the service of a history that recognized the force of social groupings in shaping the experience and life choices of their members, but revealed the social order as a far more complex set of age, sex, and professional groupings than simple Marxist models of class analysis allowed. At the same time, her work insisted upon the no less significant power of religious symbols and ideologies in shaping collective behavior and rejected the attempt to reduce these to the expression of putatively deeper economic or social interests. In subsequent articles and books, Davis displayed an ever generous receptivity to new intellectual influences: successively, French folklore studies; English Marxist work on collective action; nascent women's history; the cultural anthropology of the seventies; Italian microhistory; and literary theory. With time, the socio-economic focus of her early work gave way to a sociocultural history in which the cultural element became ever more autonomous. But the varied intellectual influences that she absorbed were always brought into dialogue with extensive archival and library research carried out with great methodological imagination, giving her work a rootedness in the sources and a technical virtuosity that specialists could not fail to appreciate. By the later years of her career, her influence had come to be felt far beyond the confines of American Reformation scholarship. As of 1993, books of hers had been translated into nine languages. One of most successful recent American manifestos for a "new" history, Lynn Hunt's 1989 *New Cultural History*, invoked her as a patron saint alongside Clifford Geertz, Michel Foucault, and E. P. Thompson.¹⁹

Within American Reformation history, Davis's work of the 1960s and 1970s joined with a variety of imported influences to generate a move toward what quickly began to be labeled the social history of the Reformation. Bernd Moeller's *Imperial Cities and the Reformation* (1962; English translation 1972) pushed

historians to see the German Reformation as an “urban event.” The simultaneous appearance in 1971 of Keith Thomas’s *Religion and the Decline of Magic* and Jean Delumeau’s *Catholicism between Luther and Voltaire* (English translation 1977) proved still more important. Each at once articulated a bold new interpretation of the course of religious change over a long Reformation era and illustrated new methods that could be used to recover the religious practice of ordinary believers—in Thomas’s case, the wide reading through an anthropological lens of a range of printed sources and court records; in Delumeau’s case, the methods of serial and quantitative history of the *Annales* school and the religious sociology of Gabriel Le Bras.²⁰ John Bossy’s neo-Durkheimian work soon added still another, often stimulatingly contradictory, perspective of like ambition and subject matter.²¹ Together, these works defined nothing less than a vast new research program for the field. In addition to recovering the theology of the reformers in all its original richness and accounting for the political history of the Reformation, Reformation history would now involve charting the long-term shifts in the character of parish-level religious practice throughout Europe from the fifteenth through the eighteenth centuries.

Coming at a time when so many other currents within American historiography and life were also promoting “history from below,” the social history of the Reformation proved hard to resist. Although many scholars, especially within divinity schools, held firm to older methods and preoccupations, such leading historians of theology as Heiko Oberman and Steven Ozment proclaimed themselves converts and altered the focus of their work. Students from thoroughly secular backgrounds perceived in the field the fascination of studying world-views scarcely less alien to them than those of the Hopi or the Azande, but of undeniable centrality for European history. Their entry into the field altered the sociology of its recruitment and weakened the influence of filiopietistic and confessional impulses. Change was most dramatic in the study of the French Reformation, where Davis’s work inspired a spate of other studies of early Protestantism and religious violence, and in the study of the Counter-Reformation, which suddenly became one of the most active areas of American scholarship. This latter subject attracted both non-Catholics inspired by Delumeau and Bossy to examine the impact of the Counter-Reformation on local religious life, and Catholics formed in the more cosmopolitan intellectual outlook of Catholic universities after 1960 and eager to reexamine their post-Tridentine heritage in the wake of Vatican II.²²

Within the Germanocentric Protestant core of the field, anthropological sensitivities or the techniques of serial history advanced more slowly. Those in this area continued to orient themselves to the debates and preoccupations of German Reformation scholars, who largely ignored the methods of French religious history and shunned folklore studies because of the political associations they had assumed during the Nazi period. Processes of long-term religious change that historians working in the Franco-Anglo-American historiographic triangle described through quantitative appraisals of shifting tendencies or the exegesis of contrasting religious styles were consequently cast by American historians of the

German Reformation as questions of whether the Reformation was “good for women” or a “success or failure.” The predictable debates that ensued rarely transcended the simple terms in which they were originally framed.²³

American historians of the German Reformation nonetheless contributed important elements to the examination of the appeal and dynamics of the early evangelical movement. This has been perhaps the central focus and greatest achievement of the past generation of German Reformation scholarship. At the same time these historians have begun to engage with more recent German theses about the dynamics of “confessionalization.”²⁴ Sixties-inflected fascination with popular movements and the dynamics of radicalism also combined with the tradition of study founded by Bender to make the historiography of Anabaptism a continuing locus of important discoveries. The last generation’s work has made evident the confessional character of Bender’s vision of the subject, set the different traditions of Anabaptism more firmly within the reform aspirations and millenarian dreams of the late middle ages and early evangelical movement, and laid bare the political dynamics that changed groups that originally aspired to transform all society into sects comprised of only those willing to undergo adult baptism.²⁵ At the same time, the impressive tradition of scholarship on late medieval theology and its connections with the Reformation that was gathering steam under the impetus of Heiko Oberman and his students in the 1960s has lost momentum.

Many Reformation specialists now stand in a very different personal relation to their subject than did their predecessors. Over the past thirty years, international Reformation scholarship has seen the advance of a widely shared, largely ecumenically inspired concern among historians of all denominations to study and appreciate traditions other than their own. Together with the discovery of many aspects of post-Reformation Catholic piety that promoted greater literacy, more systematic habits of self-discipline, and tighter codes of morality among the laity, this has led to an emphasis on the parallel consequences of the “two Reformations” and to the rejection of long-entrenched views that supposed a privileged link between Protestantism and modernity—a modest contribution of Reformation scholarship to the weakening salience of confessional difference in contemporary America. With the continuing advance of secularization and more than a generation of work in socioeconomic history built around the preindustrial/industrial dichotomy, most current Reformation scholars also now have an even stronger sense than did Preserved Smith and his peers that the age of the Reformation was less the origin of the modern world than a “world we have lost.”²⁶ Yet the motives drawing historians to study the subject remain varied. Confessional agendas have not entirely disappeared, and certain historians continue to find in their subject matter values that they see as a possible source of continuing inspiration—witness Steven Ozment’s sympathetic evocation of the loving patriarchalism that he finds in the writings about the family of the Protestant reformers, or the closing sentences of Elizabeth Gleason’s recent biography of Gasparo Contarini: “Contarini can be a wonderful partner in a dialogue with modern interlocutors who care about questions of political and religious order, of liberty

and authority. His thought still invites them to meditate on unresolved issues and on thinkable alternatives to the course of events in church and state, then and now."²⁷

Whether moved by a sense of the anthropological otherness of sixteenth-century Christianity or of its potential relevance for modern life and belief, most contemporary American historians of the Reformation nonetheless seem to share a confidence in the vitality of their field. "There is no field of historical study today that is more alive with change and fresh ideas than that of Reformation Europe," Ozment began his 1982 *Reformation Europe: A Guide to Research*.²⁸ A powerful and coherent new research program concerning the story of parish-level religious practice has recently expanded the agenda of questions and the repertoire of methods, while each year brings new monographs that help flesh out the emerging story. The recovery of the full complexity of the early evangelical movement and the concern to root out the many confessional agendas that once controlled so much Reformation historiography have led to major shifts in the interpretation of central elements in the established narrative of Protestantism's growth and institutionalization. If fewer contemporary Reformation historians see their subject as one of the birthpoints of modernity, most still see it as confidently as ever as one of the central transformations within preindustrial Europe, with broad implications not simply for the history of European ecclesiastical institutions, theology, and high politics but also for local religious life, literacy, family and gender relations, and social discipline. In this, the situation of Reformation history contrasts markedly with that of its erstwhile alter ego, Renaissance history. The Reformation, far more than the Renaissance, was a movement of ideas that swept up large elements of the European population and ushered in changes with broad implications for many aspects of religious, political, and social life. Reformation history could consequently absorb the historiographic movement of the past generation toward a more broadly inclusive history and retain the sense of connection with the narrative that initially gave the field its significance within the American history curriculum. Renaissance history could not.

THE REST OF THE FIELD

American scholarship about aspects of early modern European history other than the Reformation has always been characterized by far less thematic and institutional coherence. With some distance, however, it is possible to discern considerable continuity in the central preoccupations of American historians studying this era from the middle of the nineteenth century through the 1960s. The absorption of the new influences associated with the *Annales* school and the historians around *Past and Present* then expanded the scope of the field. The advance of research within long-established sectors modified the content of some of the older stories told about the period. Yet the majority of specialists continue to focus their research on the political and institutional history of England and

France. This continuity bespeaks the surprising durability of many old structures and assumptions.

The theme that long dominated American interest in the seventeenth and eighteenth centuries was that of the variegated evolution of European governmental forms and practices over this period, with the contrast between the gradual growth of constitutional government in England and the rise and fall of absolutism in France forming the heart of the story. For the better part of the century, whether approached with primary emphasis on the political and biographical dimension (as in the work of Conyers Read and John B. Wolf), on the institutional dimension (as in the work of Wallace Notestein), or on the dimension of political theory (as in the work of Charles McIlwain, William Farr Church, and Caroline Robbins), important American scholarship about these centuries centered around the rise of Parliament and the theorization of liberty in England and the rise and fall of absolutism in France.²⁹ Not only did this story provide a critical element in the genealogy of American politics and institutions; the many twentieth-century threats to the survival of representative government gave it continued topicality from the era of fascism's rise through the Cold War. The ideological polarization of World War II and the Cold War also bred a sense of kinship with the diplomatic and political intrigues of that earlier era of ideological polarization, the late sixteenth century, inspiring Garrett Mattingly's best-selling 1959 classic of narrative history, *The Armada*, and research by his students into the role of Geneva and Spain in destabilizing French domestic affairs.³⁰

Another important current of American historiography about this period dedicated itself to intellectual history. White, Draper, and the Englishman W. E. H. Lecky first shaped certain of the themes that American historians of this subject would explore. James Harvey Robinson launched its fortunes within the curriculum with his course *The Intellectual History of Western Europe* at Columbia in 1904. From Lynn Thorndike's eight-volume *History of Magic and Experimental Science* (1923–58), Carl Becker's *The Heavenly City of the Eighteenth-Century Philosophers* (1932), and A. O. Lovejoy's *The Great Chain of Being* (1936) through Richard Popkin's *The History of Scepticism from Erasmus to Descartes* (1963) and Peter Gay's *The Enlightenment: An Interpretation* (1967), a series of eminent early modernists attempted to trace the complex mixture of continuity and innovation that marked the history of early modern thought. With the development of the history of science (the History of Science Society was founded in 1924, but the great growth of the field came after World War II, as anxieties about the frightening power of modern technology and the need to bridge the gulf between C. P. Snow's "two cultures" fueled massive support), a substantial body of specialists in that field added their contribution to the story.³¹

Perhaps the most novel addition to the menu of scholarly concerns in the first part of the twentieth century was the rise of economic history. Although located uneasily between departments of economics and of history and slow to develop an autonomous professional society, the subject was widely taught by the first decades of the century, thanks largely to the influence of Harvard's well-connected Edwin Gay, professor of economic history from 1906 to 1936 with

time out for service as dean of the Business School, government war service, and the editorship of the *New York Post*. Entry into economics departments dominated by the ahistorical and theoretico-deductive predilections of the neoclassical school was gained largely by accepting a de facto division of labor. Premodern economic behavior was construed to differ fundamentally from modern in being shaped as much by values and institutions as by rational economic calculation. It was hence deemed suitable for inductive, historical investigation, while the contemporary economy was left to neoclassical modelbuilding. Gay was also concerned to promote the accumulation of long-term statistical series about such matters as prices and wages that might aid in the formulation of economic policy. Such concerns and assumptions attracted attention to the early modern centuries and inspired work centered on institutional structures, economic doctrines, and long-term movements of wages and prices, notably Abbott Payson Usher's still admired 1913 study of the French grain trade, Julius Klein's work on the Mesta, Earl J. Hamilton on American treasure and the price revolution, and Charles Woolsey Cole on French mercantilism.³²

Some measure of the extent to which these long-standing patterns of interest have been modified in the past generation may be obtained from a quantitative breakdown of the articles about this period that American-based historians have published since the early part of the century in four leading professional journals. The exercise has its pitfalls, for the advance of specialization has bred a proliferation of journals devoted to geographic or topical subfields, with the result that even those journals that have sought to maintain a catholicity of subject matter and approach have become more narrowly typecast. In the past decades, American scholars have also published more in the most prestigious foreign journals, a mark of the growing internationalization of scholarship and the increased respect abroad for American research. To minimize the distortions introduced by these trends, four journals of a broad, relatively nonspecialized character, two American and two European, have been sampled at regular intervals: *The American Historical Review*, *The Journal of Modern History*, *Annales*, and *The Historical Journal*. The sample may still underestimate the expansion of the discipline into new subject areas.

Table 2, which presents the geographic foci of American production, shows how overwhelmingly early modern "European" history in the United States has always been, and remains to this day, the history of certain larger European nations, particularly England and France. Spain and the Netherlands captured the attention of Prescott and Motley in the nineteenth century, and from R. B. Merriam through Richard Kagan and Simon Schama, academic historians working in the United States have continued to write important books about these countries. Yet their histories have never received a level of attention commensurate to their evident importance in this period; when Kagan wished to begin his study of Spanish history in the late 1960s, he had to go to England for his doctoral training. Still more striking is the virtual absence of work on the smaller countries of Europe, despite the presence on American soil of so many immigrants from Scandinavia, Portugal, and eastern Europe. If anything, as table 2 shows, the

TABLE 2
Geographical Distribution of Articles by American Historians on European History, 1500–1789, in Four Major Historical Journals

	1900-10				1930-40				1960-70				1980-90				
	AHR	AHR	JMH	An.	Total	AHR	JMH	An.	Total	AHR	JMH	An.	Total	AHR	JMH	An.	Total
Britain	13 (46%)	11	19		30 (45%)	13	6		7	26 (43%)	5	9	3	31			48 (36%)
France	5 (18%)	4	10		14 (21%)	6	2	1	2	11 (18%)	10	16	13	3			42 (32%)
Germany & Austria	5 (18%)		3		3 (5%)		3			3 (5%)	2	8		1			11 (8%)
Spain			1	1	2 (3%)	1	1		2	4 (7%)	4	1	1				6 (5%)
Italy	2 (7%)	1	1		2 (3%)	2	1	1		4 (7%)		5					5 (4%)
The Netherlands					1 (2%)								1				1 (1%)
Belgium			1				1			1 (2%)			2				2 (2%)
Switzerland	1 (4%)																
Russia			1		1 (2%)	1	1			2 (3%)				1			1 (1%)
Scandinavia			2		2 (3%)												
Andorra			1		1 (2%)												
Armenia			1		1 (2%)												
Ottoman Empire		1			1 (2%)	1				1 (2%)							
Yugoslavia							1			1 (2%)							
"Central Europe"																	
General European	2 (7%)	1	7		8 (12%)	4	2	1	1	7 (12%)	8	3	3				14 (12%)
Total	28	18	47	1	66	28	18	2	12	60	30	42	23	36			131

Note: See table 1 for journal titles.

concentration on a select subset of European nations has only grown in the past decades, even though the concomitant increase in the number of actively publishing specialists in the field might have been expected to spawn expansion into neglected geographic areas. The rather dramatic expansion that table 2 reveals in the volume of research devoted to French history in the past decade is probably explained by the particular attractiveness of French history during the period of peak prominence for the *Annales* school from the late 1960s into the early 1980s, as well as by the simple fact that French was for long the foreign language most studied in American high schools by students with intellectual aspirations. With native French research productivity waning over the same decades, due to a long dearth of new faculty positions and the redirection of energy by established historians toward satisfying the intense appetite for history of the larger French-reading public, by the 1980s a considerable amount of the most important archival investigation of French history was being written across the Atlantic.³³ If the percentage of work devoted to England declined in the same decade, English history has nonetheless succeeded remarkably in maintaining itself down to the present as a distinct specialization whose representation is still required within most major history departments. For no other country is the disproportion between the amount of work devoted to this subject and the country's demographic or power-political weight within early modern Europe more evident. These patterns reveal the continued and largely unthinking continuation of the Whig pairing of England and France as central to the story of early modern Europe, the tendency of specialists to replicate their specializations through their students, the reluctance of departments to hire candidates working outside the largest and most familiar national specializations, and the persistent conviction that the study of early modern England offers essential background for the study of early America.

As table 3 shows, important changes may be discerned in the questions and themes to which American historians of this restricted range of European countries have addressed themselves. Particularly noteworthy is the shift in recent years toward social and cultural history. Within the broad sphere of political and administrative history, the attention of American scholars has also moved away from the study of high politics and diplomacy toward the study of crowd and local politics and of political culture.

Much of the shift must be linked to the reception of the new historiographic currents represented by *Past and Present* and the *Annales*. Without slighting the work of such native pioneers as Franklin Ford or Robert Forster, it is probably fair to date the arrival in force of these influences to the years between 1963, when Lawrence Stone was hired at Princeton, and 1972, when Fernand Braudel's *The Mediterranean* appeared in English translation to broad acclaim. In this period, departments such as Princeton's and Michigan's established regular faculty exchanges with the Parisian *Ecole Pratique des Hautes Etudes*. The same historian who distanced himself condescendingly from recent *Annales* work in 1968 was translating essays from the journal by 1974.³⁴ The growing numbers of those drawn to French history in this period by the lure of a "history from below" with

TABLE 3
Topical Distribution of Articles by American Historians on European History, 1500-1789, in Four Major Historical Journals

	1900-10					1930-40					1960-70					1980-90				
	AHR	AHR	JMH	An.	Total	AHR	JMH	An.	Total	AHR	JMH	An.	HJ	Total	AHR	JMH	An.	HJ	Total	
High politics, political biography	7 (30%)	3	11		14 (24%)	5	5		1	11 (20%)	3		3	6 (4%)						
Institutions, administration, law	2 (9%)	3	4		7 (12%)	3	3		4	10 (18%)	2	15	1	8	26 (19%)					
Intl. relations, diplomacy, military	3 (13%)	3	5		8 (14%)	1	2		1	4 (7%)	3		1	4 (3%)						
Crowd and local politics											1	6	1	9	17 (12%)					
Political thought, political culture	1 (4%)	1	6		7 (12%)	3			2	5 (9%)	2	6	4	10	22 (16%)					
State and society						3	1			4 (7%)	2		2	4 (3%)						
Church and religion	5 (22%)	1	2		3 (5%)		2		3	5 (9%)	4	2	1	7 (5%)						
Intellectual hist., hist. of science		1	6		7 (12%)	3		1		4 (7%)	3	8	1	2	14 (10%)					
Cultural hist.		1	1		2 (4%)						5	8	3	1	17 (12%)					
Hist. of art, music, literature													2	2 (1%)						
Economic hist., historical geography	2 (9%)	2	5	1	8 (14%)	3	2	1		6 (11%)	3	1	2	6 (4%)						
Maritime empires	1 (4%)	1			1 (2%)	1				1 (2%)	1	1		2 (1%)						
Social and demographic hist.						1	1			2 (4%)	5			11 (8%)						
Hist. of technology			1		1 (2%)	2	1			3 (5%)										
Source criticism	2 (9%)																			

Note: See table 1 for journal titles.

a particularly sophisticated Continental methodological flair were especially likely to produce works of social and cultural history themselves, and this has been the national specialization where such works have been the most abundant.³⁵ American scholars also played a vital role in introducing newer currents in social history to national historiographies whose own intellectual and political traditions largely sealed them off from such viewpoints, notably Germany and Spain.³⁶ In English history, by contrast, American scholars have been far less drawn to the newer areas of social or cultural history. Here the noteworthy trends have been the increasing tendency for leading positions in the United States to be filled with specialists imported from Britain's Thatcher-shocked universities, and perhaps a greater attachment of American-based historians of England to the traditional interpretation of that country's seventeenth-century political upheavals as landmarks in the struggle for constitutional rights. Whig traditions die hard here.

Of course, the reception of the new historiographic trends represented by *Past and Present* and the *Annales* did not occur in a vacuum. Contemporary concerns about problems of economic growth in underdeveloped societies, the hopes and fears about revolution both at home and abroad, the need felt both by many students with some experience of radical politics to understand why transformation proved harder to achieve than had initially been thought and by those who remained on the sidelines of campus activism to convince themselves of the futility of such efforts, and the powerful streak of romantic identification with the dispossessed—all facilitated the assimilation of a historiography focused on economic and demographic cycles in rural societies, the social origin of revolution and the motivation of crowd action, the lives of the poor, and society's deep, change-resistant structures. As always, the process of reception involved selective assimilation and creative appropriation. Steeped in the history of politics and reluctant to accept the full Braudelian vision of people trapped within economic and geographic structures beyond their control, many American social historians sought to avoid too sharp a divorce from *histoire événementielle*. The economic models derived from classical French political economy that informed so much *Annales* historiography appeared alien and were rarely absorbed. Lastly, a major pole of concern for American social historians would always be the crises, transformations, and catlike survival of aristocratic power throughout the early modern centuries. Indeed, J. H. Hexter highlighted the importance of studying the continuities and transformations of aristocratic power as early as 1950, and the study of the nobility became the first American bridgehead into social history, even before the larger arrival of *Annales* influences in the United States. The evident connection of this subject with the grand narratives of political development and state building, with their long-standing foregrounding of the presumed contest for power between crowns and aristocracies, accounts for the precocious interest in this topic.³⁷

As new political concerns, notably feminism and identity politics, came to the fore later in the seventies and eighties, still other new subjects and new intellectual influences commanded increasing attention among all American historians.

Here, the study of early modern Europe may have been less affected than other specializations. Table 3 subsumes articles that deal primarily with women or gender roles under the broader methodological categories of social or cultural history, but a classification scheme that put articles on these topics in a separate category would also—unsurprisingly—reveal growth in recent years. The 5 percent figure that such articles would obtain in the 1980s probably falls short of the figures that might be obtained for many other time periods and parts of the world. The impulse to recover the experience of women has manifested itself among American early modernists as among American historians working in other fields, but the quest to discover the origins and persistence of patriarchy first directed the attention of women's historians less to these early modern centuries, which were marked by only modest changes in women's status and few organized struggles for women's rights, than to more distant or more recent eras.³⁸ Michel Foucault's dramatic rise to the top of the citation charts in the 1980s—he topped the *Social Sciences Citation Index* between 1985 and 1990, after placing third between 1980 and 1985 behind Clifford Geertz and Claude Lévi-Strauss³⁹—was also accompanied by increased influence in many corners of the historical profession. While several important recent books by historians that cut through this period, most notably Thomas Laqueur's *Making Sex: Body and Gender from the Greeks to Freud* (1990), show a strong Foucaultian influence, such tendencies again appear relatively muted among early modern historians. This might initially seem surprising, since so much of Foucault's earlier work focused heavily on this period, but that in fact probably explains much of this situation. The great surge of interest in Foucault's writings across the historical profession came with his *power/knowledge* essays (translated 1980), whose radical critique of disciplinary structures of knowledge meshed perfectly with feminist and multiculturalist politics of group assertion, and with his subsequent work on the history of sexuality, which energized the emerging field of gay and lesbian history. Well prior to that time, however, early modernists had been assaying his writings about the history of madness and the structures of Western thought and subjecting them to sharp empirical criticism. Also contributing to the relative weakness of Foucaultian influence in this field was the relative scarcity of interdisciplinary networks linking historians to literary scholars, New Historicist literary study having been Foucault's chief point of entry into the American academy.⁴⁰

Table 3 reveals the recent shift toward social and cultural history, but it also suggests considerable continuity in the broadest thematic preoccupations of American early modernists. If a small but important subset of American early modernists always devoted themselves to economic history, the same continues to be true today. Reinvigorated by new methods for reconstructing local economies on a quantitative basis, their monographic research has focused primarily on the actual performance of individual industries, merchant communities, or regional economies, rather than on institutional structures or economic doctrines. But their contribution within the international community of economic historians to the past generation's enormous growth of detailed local knowledge about the preindustrial European economy has been far less important than the

broader models they have articulated to characterize the major changes in the structure of the European economy in what is now seen as the long, slow run-up to the technological breakthroughs of the late eighteenth century. Franklin Mendels's model of "proto-industrialization," Immanuel Wallerstein's "world systems," Robert Brenner's neo-Marxist interpretation of capitalist agriculture, and Jan de Vries's complex vision of multiple reorganizations within the internal structure of the European economy have largely set the terms of international debate and research about the long-term course of economic change over these centuries.⁴¹ Here, American historians have continued to play the role that David Pinkney assigned them in 1958: generating broad synthetic interpretations based upon the combination of archival research and secondary reading.

In an age when the study of Latin continues to wane in America, the high level of skill in the classical languages required by the daunting erudition of so many sixteenth- and seventeenth-century thinkers has created severe barriers to entry to the field of early modern intellectual history. Nevertheless, another small but internationally respected band of holdouts has continued to cultivate this garden. Inspiration and reinforcements have often come from the ample ranks of the historians of science.⁴² At the heart of most of this work, one can still see the long-standing preoccupation with tracing the elaboration across these centuries of various forms of critical rationalism—now done, however, in a far less celebratory mode, and with an intense concern to avoid anachronism. Some prominent historians of science have carried sociologizing programs to the point where the central ambition of their work has become to show that the triumph of central elements of the new science depended fundamentally on networks of power or cultural values—not simply, or even primarily, their superior explanatory power or evidentiary basis. Their work in turn has sparked withering criticism. The debates bursting out over these issues form part of the larger contemporary battles surrounding the cultural authority of science and show how significant the historical interpretation of this era remains for the larger assessment of Western thought.⁴³

Above all, it remains the case that the majority of all research devoted by American historians to the seventeenth and eighteenth centuries still concerns political and institutional history. True, fewer American scholars have recently felt the attractions of royal or ministerial biography, high politics, and international diplomacy. Instead, their attention has shifted toward exploring the links between politics and administration on the one hand, and society or culture on the other. The macrosociological tradition of Barrington Moore, Charles Tilly, and Perry Anderson, the work of Roland Mousnier and his pupils in France on the social origins and recruitment of Old Regime administrative corporations, and the no less influential studies by Hans Rosenberg and Francis Carsten of the interaction of princes, parliaments, and protobureaucracies within the Holy Roman Empire have all in their different ways directed much American attention to the actual workings of the different component parts of early modern government and administration, to the recruitment of their personnel, and to the broader relationship between state and society.⁴⁴ The reinvigoration of the his-

tory of political thought by the theoretical writings of Quentin Skinner and J. G. A. Pocock, as well as the broader linguistic turn within American historical writing, has stimulated considerable exploration of "political culture."⁴⁵ Meanwhile, the tradition of Ernst Kantorowicz's brilliant analyses of political ceremonial was maintained across two scholarly generations by Ralph Giesey and his pupils, who between 1960 and 1986 produced an important series of studies of the ritual practices of the French monarchy that constitute perhaps the most distinctive American school within this field.⁴⁶ In different ways, all these newer concerns can be seen as partaking of the larger rejection of the classical emphasis on the actions of political leaders in favor of the examination of deeper structures, recurring patterns of behavior and thought, and collective agency that are so much a part of broader trends within the past generation's historiography. The driving force behind this research has nonetheless remained that perduring concern of American historians of this era: the effort to lay bare the character and chronology of the movement toward either the loosening or the tightening of the restraints on autocratic power in the major states of western Europe.

Many particulars of the early modern political landscape now look different than they did a generation ago. The insights and analytical vocabulary of Marx, Weber, and Otto Hintze have become part of the working apparatus of American scholars. The theme of the growth of the state occupies ample room within the broad narrative of political development alongside the older stories of the rise and fall of different countries in the international arena and the evolution of their internal constitutional arrangements—evidence of increased appreciation that the sheer power of modern governments is one of the most basic phenomena of the contemporary world whose history demands illumination. Interpretations that emphasized the link between the growth of monarchy and the rise of the bourgeoisie have given way to an appreciation of the continuing influence of a transformed nobility, the importance of warfare in promoting institutional innovation, and the coexistence of bureaucratic and patrimonial forms of administration within early modern government. Above all, the now abundant evidence of the force of representative assemblies in many parts of Europe across the early modern period, of the limitations on the power of even so paradigmatic an "absolute monarch" as Louis XIV, and of the fact that the various currents of thought advocating mixed constitutions or republicanism in the "age of the democratic revolutions" were of many national pedigrees and often considerable antiquity has called into question the old Whig themes of the distinctiveness of English constitutional evolution and its exceptional importance for the larger study of European liberty.

These lessons have been obscured at times by the division of those who specialize in the history of early modern government and politics among so many national and chronological subspecialties. The tendency toward fragmentation has reached the point where even those concerned with adjacent centuries of the same national history can lose touch with one another. Many historians of eighteenth-century French political culture, for instance, currently organize their work around the breakdown of a political culture that they depict

as monolithically and self-consciously absolutist under Louis XIV, while their counterparts specializing in seventeenth-century French government emphasize the many compromises the Sun King was forced to make with powerful groups within the kingdom and the absence of any systematic absolutist project.⁴⁷ Together with the neglect of so many parts of the European political map, this advancing specialization has impeded the establishment of convincing continent-wide syntheses of the evolution of government and political thought over the course of this period. Those who have done the best job of seeing the forest for the trees have often been social scientists coming to the field from the outside, such as Nannerl Keohane and Brian M. Downing.⁴⁸ Downing has recently demonstrated that considerable order can be brought to the political and institutional history of Europe in this period by writing it around the theme of why certain regions were able to check more successfully than others the powerful tendencies toward autocracy created by the great growth in the size of the continent's armies. The collective history patronized by the new European Science Foundation on the origins of the modern state in Europe demonstrates that constructing a genuinely continentwide political and institutional history is very much the order of the day in a Europe caught between movements toward greater integration and toward resurgent regionalism and nationalism, but American historians had little role in or influence on this project.⁴⁹ It remains to be seen whether or not U.S. historians, to whom it once came naturally to think about European history as a whole, will be able to overcome current tendencies toward national specialization and capture more attention in continuing discussions of this topic.

With the exception of the well-structured area of Reformation history, the situation of those American early modernists investigating the era's political, constitutional, and administrative history—divided among themselves into congeries of specialists on different periods and countries, yet perhaps stumbling together toward a more coherent history of European state formation—is in many ways emblematic of the larger field of early modern European history in America. In the past generation, American Europeanists have largely renounced the function of offering in their writings broad, synthetic interpretations of European history, embracing instead an increasingly zealous commitment to detailed archival research in dialogue with the historians native to the countries about which they write. In tandem with the broader tendencies promoting the advance of specialization within modern academia, this has led to ever greater fragmentation around national and thematic subcommunities, each with its own local debates. Some American specialists may even feel that it has levitated them into a curious liminal space, midway between two or more national cultures. And yet, most American research still clusters around what have always been the great stories of this period: the economic developments that prepared the ground for Europe's escape from the constraints of a preindustrial economy; the elaboration of different modes of secular rationality and their complex relationships with the continued survival of organized religion; and the survival of traditions of representative government and a reign of law in the face of powerful impulses making for increased autocracy and state power. Work in the newer areas of social, cul-

tural, or women's history often clusters around other, sometimes much debated, grand narratives: the emergence of modern family arrangements, the advance of social discipline, and the reconfigurations of social and sexual hierarchies. For one powerful force continues to counteract the tendencies toward fragmentation and uprooting: the undergraduate teaching function. In the classroom, American historians of early modern Europe still need to generate broad narratives capable of illuminating the central developments of these centuries in a manner that captures the attention of successive generations of students. So long as their narratives can accommodate the swing toward a more socially inclusive, structural history of the sort that has become the common feature of all the most important new currents of history of the past generation—as the central narratives of the Reformation and early modern period have proven capable of doing—the classroom experience continues to nudge American research toward problems that in some way or another are suggested by these narratives, and that contribute to their further refinement.

NOTES

This paper profited greatly from the discussions, formal and informal, at Providence and San Marino in connection with the conference entitled *The State of Historical Writing in North America*. Also helpful was a lively discussion at Harvard's Workshop in Early Modern British and European History. I would further like to thank Francisco Bethencourt, Diogo Curto, Jonathan Dewald, and Brad Gregory for their observations on earlier drafts of this essay, many of which I have incorporated into this version.

1. David H. Pinkney, "The Dilemma of the American Historian of Modern France," *French Historical Studies* 1 (1958): 11–25; "Time to Bury the Pinkney Thesis?" *French Historical Studies* 17 (1991): 219–23.

2. For the purposes of this paper, I shall define as American scholarship all work produced by individuals while holding positions at American universities, but I shall accord greater weight to those who received their higher education in the United States. Imperialistically but not, I believe, unjustifiably, I also include American-educated scholars teaching in Canadian universities.

3. Leonard Krieger, "European History in America," in John Higham et al., *History: The Development of Historical Studies in the United States* (Englewood Cliffs, N.J., 1965), pp. 235, 238–54.

4. *Catalogue of the Officers and Students of Dartmouth College for the Academical Year 1850–51*, p. 24; *Catalogue of Dartmouth College 1857–1858*, p. 30; *Catalogue of Dartmouth College 1863–1864*, p. 30; *Register of the University of California 1870–1871*, p. 56; *Register of the University of California 1874–1875*, p. 46; *Catalogue of the Officers and Students in Yale College 1872–1873*, p. 53.

5. Successive lectures in the course that J. Lewis Diman taught at Brown University in 1880–81 took students from "The Italian Renaissance" and "The European State System" through "The Reformation"; "Civil Wars in France, Wars in the Netherlands and the Thirty Years War"; "The Rise of Monarchy in France," "Limited Monarchy in England," "The European Colonial System," "The Balance of Power," and "Modern Political Theories," before culminating with "The Constitutional History of the United States." Brown

University Archives, Student Lecture Notes, Ms 1M-2, lecture notes of Charles Evans Hughes on modern history 1880–81.

6. These observations about the growth of the history curriculum are based upon an examination of the course catalogues of five institutions: Brown, California, Chicago, Dartmouth, and Yale. I have also found useful Richard Hofstadter, "The Department of History," in R. Gordon Hoxie et al., *A History of the Faculty of Political Science, Columbia University*, The Bicentennial History of Columbia University (New York, 1955), pp. 207–49. Beyond the rapid establishment of courses on the Renaissance and Reformation, English history, and the French Revolution, these reveal for the first decades of the twentieth century an occasional continentwide survey of the seventeenth and eighteenth century with a title such as "The Political and Military History of Europe from 1618 to 1763" or "Europe by Treaty from 1648 to 1789," as well as some nation-specific courses devoted to France, Spain (a curricular fixture at California from 1909–10 onward, but otherwise rare), and (surprisingly frequently) Prussia. The number of courses on the rise of Prussia testifies both to the important links between American and German historical scholarship in this period and to a broader fascination with the growth of a powerful new nation in the heart of Europe.

7. Henry Warner Bowden, *Church History in the Age of Science: Historiographical Patterns in the United States 1876–1918* (Chapel Hill, 1971), pp. 58–68, 239–45. That the assistant secretary at the Smithsonian who oversaw the operation of congressionally funded periodicals was Jewish furthered the disaffection of at least one prominent ecclesiastical historian, Samuel Macauley Jackson, who led the secession of the church historians. In submitting for publication the paper of one colleague, he added that if it were rejected (as it subsequently was) "it will be incumbent upon me to announce to my clerical friends whom I ask to write papers for the Association that their papers will not be published because a Jew says they must not be!"

8. James Harvey Robinson, "The Study of the Lutheran Revolt," *American Historical Review* 8 (1903): 205–16; Jacob Salwyn Schapiro, *Social Reform and the Reformation* (New York, 1909); Preserved Smith, "Luther's Early Development in the Light of Psychoanalysis," *American Journal of Psychology* 24 (1913): 360–77; Hartmut Lehmann, *Martin Luther in the American Imagination* (Munich: W. Fink, 1988), pp. 211n, 227; Henry Warner Bowden, *Church History in an Age of Uncertainty: Historiographical Patterns in the United States, 1906–1990* (Carbondale, Ill., 1991), p. 3.

9. I. Bernard Cohen, *Revolution in Science* (Cambridge, Mass., 1985), pp. 391–96; H. Floris Cohen, *The Scientific Revolution: A Historiographical Inquiry* (Chicago, 1994).

10. Preserved Smith, *The Age of the Reformation* (New York, 1920); George Park Fischer, *The Reformation* (New York, 1873).

11. Higham, *History*, chap. 5; Sydney Ahlstrom, "Continental Influences on American Christian Thought since World War I," *Church History* 27 (1958): 256–72; James M. Stayer, "The Eclipse of Young Man Luther: An Outsider's Perspective on Luther Studies," *Canadian Journal of History* 19 (1984): 167–82.

12. The character and preoccupations of much Catholic historical writing in America can be inferred by surveying the early decades of the *Catholic Historical Review*. See also Hubert Jedin and John Dolan, eds., *Handbook of Church History* (New York, 1965); and, for the broader context, George M. Marsden, *The Soul of the American University: From Protestant Establishment to Established Nonbelief* (Oxford, 1994); Peter Novick, *That Noble Dream: The "Objectivity Question" and the American Historical Profession* (Cambridge, 1988), pp. 69n, 172–74, 364–66; Philip Gleason, "American Catholic Higher Education, 1940–1990:

The Ideological Context," in George M. Marsden and Bradley J. Longfield, eds., *The Secularization of the Academy* (Oxford, 1992), pp. 234–58.

13. Samuel M. Jackson, *Huldreich Zwingli* (New York, 1901); Williston Walker, *John Calvin, the Organizer of Reformed Protestantism* (New York, 1906); James Westfall Thompson, *The Wars of Religion in France 1559–1576* (Chicago, 1909).

14. William Haller, *The Rise of Puritanism* (New York, 1958); M. M. Knappen, *Tudor Puritanism: A Chapter in the History of Idealism* (Chicago, 1939).

15. Roland Bainton, *David Joris, Wiedertäufer und Kämpfer für Toleranz im 16. Jahrhundert* (Leipzig, 1937); idem, *Hunted Heretic: The Life and Death of Michael Servetus, 1511–1553* (Boston, 1953); George Huntston Williams, *The Radical Reformation* (Philadelphia, 1962).

16. Bender's most important publications were "The Anabaptist Vision," *Church History* 13 (1944): 3–24; and *Conrad Grebel, 1498–1526: The Founder of the Swiss Brethren* (Goshen, 1950). For his life, work, and context: *The Mennonite Quarterly Review* 38 (1964), "Harold S. Bender Memorial Number"; James C. Juhnke, *Vision, Doctrine, War: Mennonite Identity and Organization in America 1890–1930* (Scottsdale, 1989), esp. pp. 277–82.

17. A. G. Dickens and John M. Tonkin, *The Reformation in Historical Thought* (Oxford, 1985), chap. 9, "Rediscovered Dimensions: The Reformation Radicals."

18. Personal communication from Natalie Zemon Davis, March 10, 1995. Many valuable biographical details about Davis may also be found in MARHO, The Radical Historians Organization, *Visions of History* (New York, 1984), pp. 100–122; and now *A Life of Learning: Natalie Zemon Davis: Charles Homer Haskins Lecture for 1997*, American Council of Learned Societies Occasional Paper 39.

19. The evolution of Davis's interests can be followed most clearly through these works: "A Trade Union in Sixteenth-Century France," *Economic History Review* 19 (1966): 48–69; *Society and Culture in Early Modern France* (Stanford, 1975); "The Sacred and the Body Social in Sixteenth-Century Lyon," *Past & Present* 90 (1981): 40–70; *The Return of Martin Guerre* (Cambridge, Mass., 1983); *Fiction in the Archives: Pardon Tales and Their Tellers in Sixteenth-Century France* (Stanford, 1987). Longer analyses and appreciations of the central themes of her work may be found in Barbara B. Diefendorf and Carla Hesse, "Introduction: Culture and Identity," in Diefendorf and Hesse, eds., *Culture and Identity in Early Modern Europe (1500–1800): Essays in Honor of Natalie Zemon Davis* (Ann Arbor, 1993), which includes a full bibliography of her publications; and Suzanne Desan, "Crowds, Community, and Ritual in the Work of E. P. Thompson and Natalie Davis," in Lynn Hunt, ed., *New Cultural History* (Berkeley, 1989).

20. Keith Thomas, *Religion and the Decline of Magic* (New York, 1971); Jean Delumeau, *Le Catholicisme entre Luther et Voltaire* (Paris, 1971). Delumeau's work was translated into English, with an important foreword by John Bossy, in 1977.

21. John Bossy, "The Counter-Reformation and the People of Catholic Europe," *Past & Present* 47 (1970): 51–70; "The Social History of Confession in the Age of the Reformation," *Transactions of the Royal Historical Society*, 5th ser., 25 (1975): 21–38; "Essai de sociographie de la messe, 1200–1700," *Annales: E.S.C.* 36 (1981): 44–70; *Christianity in the West 1400–1700* (Oxford, 1985).

22. For just some of the most important illustrations of these tendencies, see Philip Benedict, *Rouen during the Wars of Religion* (Cambridge, 1981); Philip T. Hoffman, *Church and Community in the Diocese of Lyon, 1500–1789* (New Haven, 1984); Ronald Po-Chia Hsia, *Society and Religion in Münster, 1535–1618* (New Haven, 1984); Barbara B. Diefendorf, *Beneath the Cross: Catholics and Huguenots in Sixteenth-Century Paris* (Oxford, 1991);

Marc R. Forster, *The Counter-Reformation in the Villages: Religion and Reformation in the Bishopric of Speyer, 1560–1720* (Ithaca, 1992); Sara T. Nalle, *God in La Mancha: Religious Reform and the People of Cuenca, 1500–1650* (Baltimore, 1992); Philip M. Soergel, *Wondrous in His Saints: Counter-Reformation Propaganda in Bavaria* (Berkeley, 1993); John W. O'Malley, *The First Jesuits* (Cambridge, Mass., 1993); Elizabeth G. Gleason, *Gasparo Contarini: Venice, Rome and Reform* (Berkeley, 1993).

23. As historians working in this field are beginning to recognize. See Bruce Tolley, *Pastors and Parishioners in Württemberg during the Late Reformation 1581–1621* (Stanford, 1995), p. 86; and the review of this book by Gerald Strauss, *American Historical Review* 101 (1996): 1231. For the contrast between the different patterns of analysis, cf. Natalie Z. Davis, "City Women and Religious Change," in Davis, *Society and Culture*, and Steven Ozment, *When Fathers Ruled: Family Life in Reformation Europe* (Cambridge, Mass., 1983); Bernard Vogler, *Vie religieuse en pays rhénan dans la seconde moitié du XVI^e siècle (1556–1619)* (Lille: Service de Reproduction des Thèses, 1974), and Gerald Strauss, "Success and Failure in the German Reformation," *Past & Present* 67 (1975): 30–63; James M. Kittelson, "Successes and Failures in the German Reformation: The Report from Strasbourg," *Archiv für Reformationsgeschichte* 73 (1982): 153–75; Geoffrey Parker, "Success and Failure during the First Century of the Reformation," *Past & Present* 136 (1992): 43–82.

24. Steven Ozment, *The Reformation in the Cities* (New Haven, 1975); Paul A. Russell, *Lay Theology in the Reformation: Popular Pamphleteers in Southwest Germany, 1521–1525* (Cambridge, 1985); Carlos M. Eire, *War against the Idols: The Reformation of Worship from Erasmus to Calvin* (Cambridge, 1986); Mark U. Edwards, Jr., *Printing, Propaganda and Martin Luther* (Berkeley, 1994); Ronald Po-Chia Hsia, *Social Discipline in the Reformation* (London, 1989); Paula Sutter Fichtner, *Protestantism and Primogeniture in Early Modern Germany* (New Haven, 1989).

25. The reinterpretation of Anabaptism in the past generation has involved European and North American scholars alike. See in particular James M. Stayer, *Anabaptists and the Sword* (Lawrence, Kans., 1972); K. Deppermann, W. O. Packull, and Stayer, "From Monogenesis to Polygenesis: The Historical Discussion of Anabaptist Origins," *Mennonite Quarterly Review* 49 (1975): 83–121; Hans-Jürgen Goertz, ed., *Umstrittenen Taufertum 1515–1975: Neue Forschungen* (Göttingen, 1975); Deppermann, *Melchior Hoffman* (Göttingen, 1979); Stayer, *The German Peasants' War and Anabaptist Community of Goods* (Montreal, 1991); Claus-P. Clasen, *Anabaptism: A Social History, 1525–1618* (Ithaca, 1972).

26. For a clear expression of this view, see Thomas A. Brady, Jr., Heiko A. Oberman, and James D. Tracy, "Introduction: Renaissance and Reformation, Late Middle Ages and Early Modern Era," in *Handbook of European History 1400–1600: Late Middle Ages, Renaissance and Reformation* (Leiden, 1994), I, xiii–xvi.

27. Ozment, *When Fathers Ruled*; Elizabeth Gleason, *Contarini*, p. 301.

28. Steven Ozment, ed., *Reformation Europe: A Guide to Research* (St. Louis, 1982), p. 1.

29. Ample bibliographic indications may be found in Krieger, "European History in America."

30. Robert M. Kingdon, *Geneva and the Coming of the Wars of Religion in France, 1555–1563* (Geneva, 1955); DeLamar Jensen, *Diplomacy and Dogmatism: Bernardino de Mendoza and the French Catholic League* (Cambridge, Mass., 1964).

31. On the development of the history of science, see Arnold Thackray, "History of Science," in Paul T. Durbin, ed., *A Guide to the Culture of Science, Technology and Medicine* (New York, 1980), pp. 12–19; Arnold Thackray and Robert K. Merton, "On Discipline Building: The Paradoxes of George Sarton," *Isis* 63 (1972): 473–95.

32. Usher, Klein, and Hamilton were all students of Gay's. Abbott Payson Usher, *The*

History of the Grain Trade in France, 1400–1710 (Cambridge, Mass., 1913); Julius Klein, *The Mesta: A Study in Spanish Economic History, 1273–1836* (Cambridge, Mass., 1920); Earl J. Hamilton, *American Treasure and the Price Revolution in Spain, 1501–1650* (Cambridge, Mass., 1934); Charles Woolsey Cole, *Colbert and a Century of French Mercantilism* (New York, 1939). For the development of economic history, see Arthur H. Cole, "Economic History in the United States: Formative Years of a Discipline," *Journal of Economic History* 28 (1968): 556–89; Steven A. Sass, *Entrepreneurial Historians and History: Leadership and Rationality in American Economic Historiography 1940–1960* (New York, 1982), chap. 1; *Dictionary of American Biography*, supplement four, 1946–50 (New York, 1974), s.v. "Edwin Francis Gay."

33. Jeremy D. Popkin, "'Made in U.S.A.': les historiens français d'outre-Atlantique et leur histoire," *Revue d'Histoire Moderne et Contemporaine* 40 (1993): 317–18, provides a revealing discussion of the motivations that drew many Americans into French history in these years. For evidence of the shifting balance of research activity, see Roland Mousnier's review of Barbara B. Diefendorf, *Paris City Councillors in the Sixteenth Century*, in *Revue Historique* 271 (1984): 174; Pinkney, "Time to Bury the Pinkney Thesis?" 222–23.

34. Cf. Orest Ranum, *Paris in the Age of Absolutism: An Essay* (New York, 1968), pp. 297–98; Robert Forster and Orest Ranum, eds., *Biology of Man in History: Selections from the Annales Economies, Sociétés, Civilisations* (Baltimore, 1975).

35. A small sampling of important recent American works on the social history of Old Regime France that indicates the range of topics examined: Robert Forster, *The House of Saulx-Tavanes: Versailles and Burgundy 1700–1830* (Baltimore, 1971); Kathryn Norberg, *Rich and Poor in Grenoble 1600–1814* (Berkeley, 1985); Steven Laurence Kaplan, *Provisioning Paris: Merchants and Millers in the Grain and Flour Trade during the Eighteenth Century* (Ithaca, 1984); Cissie Fairchild, *Domestic Enemies: Servants and Their Masters in Old Regime France* (Baltimore, 1984); Robert Darnton, *The Great Cat Massacre and Other Episodes in French Cultural History* (New York, 1984).

36. Witness the pioneering stature within the respective national historiographies of works such as David W. Sabean, *Power in the Blood: Popular Culture and Village Discourse in Early Modern Germany* (Cambridge, 1984), and idem, *Property, Production and Family in Neckarhausen, 1700–1870* (Cambridge, 1990); Christopher R. Friedrichs, *Urban Society in an Age of War: Nördlingen, 1580–1720* (Princeton, 1979); Richard L. Kagan, *Students and Society in Early Modern Spain* (Baltimore, 1974), and idem, *Lawsuits and Litigants in Castile 1500–1700* (Chapel Hill, 1981); James S. Amelang, *Honored Citizens of Barcelona: Patrician Culture and Class Relations, 1490–1714* (Princeton, 1986).

37. J. H. Hexter, "The Education of the Aristocracy in the Renaissance," *Journal of Modern History* 22 (1950): 1–20, idem, "A New Framework for Social History," *Journal of Economic History* 15 (1955), and idem, "'Factors in Modern History,'" all collected in idem, *Reappraisals in History* (Evanston, 1961); Franklin L. Ford, *Robe and Sword: The Regrouping of the French Aristocracy after Louis XIV* (Cambridge, Mass., 1953); Robert Forster, *The Nobility of Toulouse in the Eighteenth Century: A Social and Economic Study* (Baltimore, 1960). The history of the early modern French nobility has subsequently been one of the subjects most extensively examined by American social historians of this period, most impressively by Jonathan Dewald, *The Formation of a Provincial Nobility: The Magistrates of the Parlement of Rouen 1499–1610* (Princeton, 1980); idem, *Pont-St-Pierre 1398–1789: Lordship, Community, and Capitalism in Early Modern France* (Berkeley, 1987); idem, *Aristocratic Experience and the Origins of Modern Culture: France, 1570–1715* (Berkeley, 1993).

38. Important books by American historians on women's history in this period include Carolyn C. Lougee, *Le Paradis des Femmes: Women, Salons, and Social Stratification in Seven-*

teenth-Century France (Princeton, 1976); Merry E. Wiesner, *Women and Gender in Early Modern Europe* (Cambridge, 1993).

39. Mark A. Schneider, *Culture and Enchantment* (Chicago, 1993), p. 188n.

40. Prominent expressions of the reserved reception accorded Foucault by American early modern historians include H. C. Erik Midelfort, "Madness and Civilization in Early Modern Europe: A Reappraisal of Michel Foucault," in Barbara C. Malament, ed., *After the Reformation: Essays in Honor of J. H. Hexter* (Philadelphia, 1980), pp. 247–66; George Huppert, "Divinatio et eruditio: Thoughts on Foucault," *History and Theory* 13 (1974): 191–207. On the broader issue of the reception of Foucault, see Schneider, *Culture and Enchantment*, p. 188; Allan Megill, "The Reception of Foucault by Historians," *Journal of the History of Ideas* 48 (1987): 117–41; Jan Goldstein, ed., *Foucault and the Writing of History* (Oxford, 1994); and Joan Wallach Scott, *Gender and the Politics of History* (New York, 1988), esp. pp. 1–11, a central text.

41. Franklin Mendels, "Proto-industrialization: The First Phase of the Industrialization Process," *Journal of Economic History* 32 (1972): 241–61; Myron P. Gutmann, *Toward the Modern Economy: Early Industry in Europe, 1500–1800* (New York, 1988), pp. 245–46, for a listing of the most important titles within the vast literature spawned by Mendels's article; Immanuel Wallerstein, *The Modern World System*, 4 vols. to date (New York, 1974–); Patrick O'Brien, "European Economic Development: The Contribution of the Periphery," *Economic History Review*, 2nd ser., 35 (1982): 1–18; Robert Brenner, "Agrarian Class Structure and Economic Development in Pre-industrial Europe," *Past & Present* 70 (1976): 30–74; T. H. Aston and C. H. E. Philpin, eds., *The Brenner Debate: Agrarian Class Structure and Economic Development in Pre-industrial Europe* (Cambridge, 1985); Jan de Vries, *The Economy of Europe in an Age of Crisis, 1600–1750* (Cambridge, 1976); idem, *European Urbanization 1500–1800* (Cambridge, Mass., 1984); idem, "The Industrial Revolution and the Industrious Revolution," *Journal of Economic History* 54 (1994): 249–70.

42. For some important recent work in this field, Anthony Grafton, *Joseph Scaliger*, 2 vols. (Oxford, 1983–93); idem, *New Worlds, Ancient Texts* (Cambridge, Mass., 1992); Grafton and Lisa Jardine, *From Humanism to the Humanities* (Cambridge, Mass., 1986); Margaret C. Jacob, *The Radical Enlightenment* (London, 1981); idem, *Living the Enlightenment* (Oxford, 1991); Alan C. Kors, *Atheism in France 1650–1729* (Princeton, 1990). The intense examination of actual scholarly and pedagogic practice that Grafton has used to revitalize the history of humanist education and classical scholarship derives from the history of the exact sciences via Grafton's Chicago teacher Noel Swerdlow. Jacob is a historian of science by training.

43. Steven Shapin and Simon Shaffer, *Leviathan and the Air-Pump* (Princeton, 1985); Shapin, *A Social History of Truth* (Chicago, 1994); Mordechai Feingold, "Essay Review: When Facts Matter," *Isis* 87 (1996): 131–39; letters to the editor by Shapin and Feingold, *ibid.*, 681–87; Mario Biagioli, *Galileo, Courtier: The Practice of Science in the Culture of Absolutism* (Chicago, 1993); Michael H. Shank, "Essay Review: Galileo's Day in Court," *Journal for the History of Astronomy* 25 (1994): 236–43; Biagioli, "Playing with the Evidence," *Early Science and Medicine* 1 (1996): 70–105; Shank, "How Shall We Practice History? The Case of Mario Biagioli's *Galileo, Courtier*," *ibid.*, 106–50.

44. Particularly important studies of this sort include James A. Vann, *The Making of a State: Württemberg 1593–1793* (Ithaca, 1985); William Beik, *Absolutism and Society in Seventeenth-Century France: State Power and Provincial Aristocracy in Languedoc* (Cambridge, 1985); John Brewer, *The Sinews of Power: War, Money and the English State, 1688–1783* (New York, 1989).

45. Important examples in different registers: J. G. A. Pocock, *The Machiavellian Mo-*

ment: *Florentine Political Thought and the Atlantic Republican Tradition* (Princeton, 1975); Dale Van Kley, *The Damiens Affair and the Unraveling of the Ancien Regime, 1750–1770* (Princeton, 1984); Marc Raeff, *The Well Ordered Police State: Social and Institutional Change through Law in the Germanies and Russia, 1600–1800* (New Haven, 1983); Sarah Maza, *Private Lives and Public Affairs: The Causes Célèbres of Prerevolutionary France* (Berkeley, 1993).

46. Ralph Giesey, *The Royal Funeral Ceremony in Renaissance France* (Geneva, 1960); idem, *Cérémonial et puissance souveraine: France, XVe–XVIIe siècles*, *Cahier des Annales* 41 (Paris, 1987); Sarah Hanley, *The Lit de Justice of the Kings of France* (Princeton, 1983); Richard A. Jackson, *Vive le Roi! A History of the French Coronation from Charles V to Charles X* (Chapel Hill, 1984); Lawrence M. Bryant, *The King and the City in the Parisian Royal Entry Ceremony* (Geneva, 1986). This school has been especially warmly received in France since the mid-1980s, when François Furet initiated a move toward a more philosophical history of politics.

47. Cf. Daniel Gordon, *Citizens without Sovereignty: Equality and Sociability in French Thought, 1670–1789* (Princeton, 1994), pp. 3–5, 40; William Beik, “Louis XIV and the Cities,” in James L. McClain, John M. Merriman, and Ugawa Kaoru, eds., *Edo and Paris: Urban Life and the State in the Early Modern Era* (Ithaca, 1994), pp. 68–85.

48. Nannerl Keohane, *Philosophy and the State in France: The Renaissance to the Enlightenment* (Princeton, 1980); Brian M. Downing, *The Military Revolution and Political Change: Origins of Democracy and Autocracy in Early Modern Europe* (Princeton, 1993).

49. At the time this volume went to press, the following volumes of this project had appeared: Richard Bonney, ed., *Economic Systems and State Finance* (Oxford, 1995); Janet Coleman, ed., *The Individual in Political Theory and Practice* (Oxford, 1996); Wolfgang Reinhard, ed., *Power Elites and State Building* (Oxford, 1996); Peter Blickle, ed., *Resistance, Representation, and Community* (Oxford, 1997); Antonio Padoa-Schioppa, ed., *Legislation and Justice* (Oxford, 1997). The comparison between this project and the most important recent American undertaking of the sort, the volumes in the *Rise of Modern Freedom* series produced under the auspices of Washington University’s Center for the History of Freedom, illustrates once again the continuing force of Whig traditions in America.